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To Thos. Callaway Esq with the
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Author

AN ACCOUNT
OF
THE CONSTRUCTION
OF THE
BRITANNIA AND CONWAY
TUBULAR BRIDGES,
WITH
A COMPLETE HISTORY OF THEIR PROGRESS,

FROM THE CONCEPTION OF THE ORIGINAL IDEA,
TO THE CONCLUSION OF THE ELABORATE EXPERIMENTS WHICH
DETERMINED THE EXACT FORM AND MODE OF CONSTRUCTION ULTIMATELY ADOPTED.

BY
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VICE-PRESIDENT OF THE LITERARY AND PHILOSOPHICAL SOCIETY, MANCHESTER, ETC.

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TO

JOHN KENNEDY, Esq.,

OF

ARDWICK HOUSE, MANCHESTER,

A GENTLEMAN LONG DISTINGUISHED BY THE INTEREST HE HAS
TAKEN IN THE PROGRESS OF MECHANICAL SCIENCE,

AND WHOSE NAME IS ASSOCIATED WITH MANY OF THE
IMPROVEMENTS OF THE AGE,

THIS WORK ON TUBULAR BRIDGES

IS RESPECTFULLY INSCRIBED

BY HIS MUCH OBLIGED AND VERY FAITHFUL FRIEND,

THE AUTHOR.

PREFACE.

In submitting the following pages to the ordeal of public criticism, I would bespeak indulgence for the many imperfections which, doubtless, will be found in the work. In arranging the first section of the book, I had two distinct objects in view; first, to establish my claim to a considerable portion of the merit of the construction of the Conway and Britannia Bridges; and secondly, to give an accurate and faithful record of all the proceedings connected with the progress of these stupendous structures, from their origin to their successful completion. The various public statements which were made at different times by different individuals, and which either entirely passed over, or concealed the real nature of the services I had rendered in connection with these undertakings, appeared, not only to me, but to my friends, to call for some public assertion of my claims; and, moreover, I conceived that the keen interest with which I had pursued the investigation, and the prominent part I had taken in the proceedings from the very commencement of my professional appointment as engineer for the bridges, to the erection of the first Conway tube, were in themselves sufficient qualifications for the task which I have undertaken in the compilation of this work.

In order to attain these objects, I have published the correspondence which took place between the parties engaged in the undertaking, in connection with a simple statement of facts and occurrences. It appeared to me, that the publication of this correspondence, with the narrative, would not only give a higher interest to the work, but would also convey to the mind of the reader, the most faithful account of each person's share of the intellectual labour evidenced in the construction of these great works. The whole correspondence has been printed almost entirely in its original form, those passages only having been struck out which did not bear upon the subject, or which appeared to have been written under circumstances of excitement, and consequently calculated by their reproduction to engender feelings of pain and regret.

I have approached this portion of the work with that hesitation which a just diffidence in my own powers could not fail to inspire; but the extent of the correspondence, taken in connection with the experimental researches which follow, will, it is hoped, speak for the industry and closeness of application with which the subject has been pursued; and also, for the active and unflinching exertions which have characterized my official connection with the undertaking.

I cannot venture to flatter myself that, in my engagement with the Chester and Holyhead Railway Company, I have adequately satisfied the various conditions under which I was placed, or avoided the dangers of an unpleasant controversy with some of my contemporaries. I however cherish the hope, that the perusal of this correspondence will establish the fact, that it was in a great degree owing to my determined perseverance, that Mr. Stephenson's original conception has been successfully carried into execution, and that the elaborate series of experiments which I performed have established the true principle upon which tubular bridges should be constructed. To this early conception I make no claim; but with regard to the services which I afterwards rendered, I must leave the estimate of their merits to the unbiased judgement of the reader.

The interest which the introduction and construction of the tubular bridges have created, will ensure for the third section of the work (which contains in detail the laborious experimental investigation) an attentive consideration on the part of the scientific reader. These researches, which have brought to light an entirely novel principle of construction, extended over a period of nearly two years, and from the advantages which I have enjoyed in having conducted a similar investigation some years since, I would venture to encourage the hope, that the results obtained from the experiments more immediately connected with these works, will not only increase our store of knowledge, but will contribute to the enlargement of our field of observation in those departments of physical truth which yet remain to be explored. In addition to the honour, which I feel, in having been selected by Mr. Stephenson as the fittest person to elucidate the subject and conduct the inquiry, I have pleasure in acknowledging the liberality which furnished the means for promoting the researches on a scale of such magnitude as to ensure conclusive results. With a more limited preliminary investigation, it is evident, that it would have been imprudent, on the part of the Chester and Holyhead Railway Company, to have entered upon a work involving an immense outlay of capital, and on which the opinions of the scientific and engineering world were so much divided.

In conclusion, I have to express my obligations to several scientific and highly valued friends, for having read over the manuscript, and suggested alterations in the narrative, and omissions in the correspondence, which have rendered the work more concise, and I trust more acceptable to the general reader. I have further to acknowledge, that I am indebted to Mr. Tate, of Battersea, for the mathematical analyses, and for the interesting and valuable deductions and formulæ, which will be found at the close of the experiments.

Manchester, June 1st, 1849.

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THE

CONWAY AND MENAI

TUBULAR BRIDGES.

IN the construction of the Chester and Holyhead Railway two formidable obstacles had to be overcome. The deep and rapid tidal streams at the Conway and Menai Straits had to be crossed by bridges, which must necessarily be of extraordinary span, and of great strength. No centerings or other substructures, such as are usually resorted to for putting such massive structures together, could be erected.

Under such circumstances the most obvious resource of the engineer was a suspension bridge; but the failure of more than one attempt had proved the impossibility of running railway trains over bridges of that class with safety. Some *new* expedient of engineering was therefore required, and an engineer bold and skilful enough to conceive such an expedient and to apply it. That engineer was found in Mr. Robert Stephenson, and that expedient is the one, the history of which it is the object of the following pages to relate.

Under Mr. Stephenson's direction numerous other schemes had been devised. Both timber and cast-iron arches had been thought of; and a model of a very handsome bridge for crossing the Menai Straits on the latter principle had been constructed, and it was, I believe, submitted to the consideration of a Parliamentary Committee. The possibility of erecting cast-iron arches over so great a span as 450 feet was however questionable, and the security of such a bridge could not but have been endangered by the great changes to which the material would have been subjected from atmospheric influences, and from the vibrations produced by the passage of heavy trains; but a more important objection even than these weighed in the withdrawal of this design. The Lords Commissioners of the Admiralty, as Conservators of the Navigation, opposed the erection of any structure which should offer a hindrance to the free passage of vessels under it, and insisted upon a clear headway of 105 feet from the level of high-water. It was under these circumstances —having to encounter extraordinary difficulties of execution, and being compelled by the opposition of so powerful a branch of the Government as the Admiralty Board, to abandon the ordinary resources of the engineer—that Mr. Stephenson conceived the original idea of a huge tubular bridge, to be constructed of riveted plates and supported by chains, and of such dimensions as to allow of the passage of locomotive engines and railway trains through the interior of it.

It was with reference to this expedient, after all others had been found inapplicable, that I was consulted by him, and that my opinion was requested, first as to the practicability of the scheme, and secondly as to the means necessary for carrying it out. This consultation took place early in April 1845, and as far as could be gathered from Mr. Stephenson at the time, his idea then was, that the tube should be either of a circular or an egg-shaped sectional form; he was strongly impressed with the primary importance of the use of chains, placing his reliance in them as the principal support of the bridge; and he never for a moment entertained the idea of making the tube self-supporting. The wrought-iron tube, according to his idea, was, indeed, entirely subservient to the chains, and intended to operate from its rigidity and weight as a stiffener, and to prevent, or at least to some extent counteract, the undulations due to the catenary principle of construction. In fact, for many months afterwards, and even up to the time of the experiments on the model tube in December 1846, he insisted, as will be seen from the annexed correspondence, on the application of such chains. A perusal of this correspondence will, moreover, show that I was throughout strongly opposed to their application, even as an auxiliary. I always felt that in a combination of two bodies, the one of a perfectly rigid, and the other of a flexible nature, there was a principle of weakness; for the vibrations to which the one would be subjected, would call into operation forces whose constant action upon the rivets and fastenings of the other could not but tend to loosen them, and thus, by a slow but sure agency, to break up the bridge.

At the period of the consultation in April 1845, there were no drawings illustrative of the original idea of the bridge, nor had any calculations been made as to the strength, form, or proportions of the tube. I was asked whether such a design was practicable, and whether I could accomplish it; and it was ultimately arranged that the subject should be investigated experimentally, to determine, not only the value of Mr. Stephenson's original conception, but that of any other tubular form of bridge which might present itself in the prosecution of my researches. The matter was placed unreservedly in my hands; the entire conduct of the investigation was entrusted to me; and, as an experimenter, I was to be left free to exercise my own discretion, in the investigation of whatever forms or conditions of the structure might appear to me best calculated to secure a safe passage across the Straits. This freedom of action was obviously necessary to the success of my experiments. I cannot but feel myself to have been honoured by that confidence in my judgement which it implied. A period of five weeks was occupied in the construction of the experimental models, and in devising the means and preparing the necessary apparatus for the experiments. The whole series, detailed in the Appendix, was conducted at my works, Millwall, Poplar, and the earlier experiments were witnessed by Mr. Henry Ross and Mr. Robert Murray, as assistants. Subsequently, when the large model tube was made, and the ultimate sectional form almost arrived at, Mr. Edwin Clarke, one of Mr. Stephenson's assistants, was also present, and for his employer's information took copious notes of the proceedings.

In consequence of the favourable opinion entertained by Mr. Stephenson of the cylindrical tubes, it was deemed expedient to commence experiments upon models of that kind, and to extend them subsequently to elliptical tubes. The whole of the results, and observations made during the progress of the researches, as well as a description of the apparatus employed, I have carefully arranged and placed as an Appendix, thinking that in that shape they would be more easy of reference to the scientific reader, who would thus be able to trace the gradual development of the principle of construction ultimately arrived at, from the experiments which led to it.

On entering upon the correspondence, I may remark, that I have in general printed the letters entire, and where selections have been made, the parts omitted usually referred to matters of a private nature. No doubt many communications are altogether missing, but I have endeavoured to connect the whole by such remarks, as will render the narrative consecutive and clear.

The following letter to Mr. Stephenson was written during the experiments on the cylindrical tubes, and it details some early reflections with reference to a form of tube intended, in a manner, to meet Mr. Stephenson's views, but which suggested itself to my mind as superior to one supported by chains.

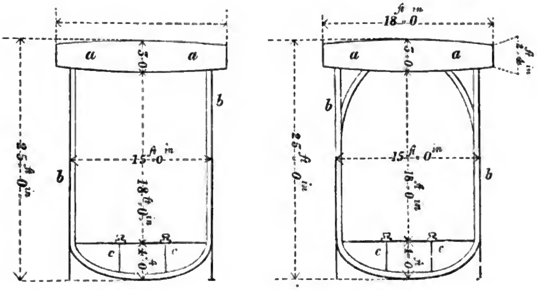
He had conceived the idea, in which I then to a certain extent concurred, that the upper side of the tube should be brought into a state of tension as well as the lower, so that its strength, although a rigid structure, should not lie, as that of a beam does, in its resistance to tension on the one side and to compression on the other, but wholly in a resistance to tension, as that of a flexible structure does. With this view, it was thought that something like the form of a catenary should be given to it, with a rigid bottom and sides.

My dear Sir, Millwall Works, May 31, 1845.

It has occurred to me, that the plate bridge might be made considerably stronger by adopting a different form of construction, and by judicious arrangement the whole might be made in one span of such an extent, as 450 feet, so as to render it self-constructive in the formation of its own scaffold or platform.

I have considered the subject with the utmost care since I left you, and after repeated comparison, I am induced respectfully to submit the following rough sketch and description for consideration.

Fig. 1.



On contrasting the forces of extension and compression with each other, I find we must place our sole reliance upon the resisting forces of a tensile strain, and to use that of compression only for the purpose of giving tenacity and rigidity to the structure. With these views, I would suggest that the whole of the upper sides of the iron tube or tunnel should sustain the load, and for that purpose I would incline to the annexed section (see fig. 1).

The parts, *a, a,* to be each 18 feet wide, 3 feet deep in the middle, and 2 feet 4 inches at each end; and composed of plates, say half an inch thick, with angle iron frames, and connecting plates at every 10 feet.

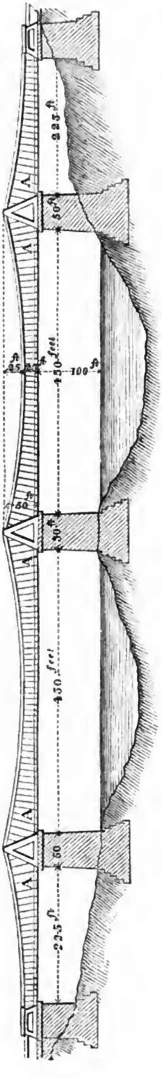
The sides, *b, b,* to be composed of $\frac{5}{16}$ (or quarter-inch patent) plates with internal ⊤ iron frames, radiating from the upper curvature of the suspending plates, as shown on the sides A, A, A (see fig. 2).

The bottom to be formed as before, with a permanent platform and rails, as represented in the sections at *c, c.* The sides, *b, b,* in this case descending a little below the curves, for the purpose of rendering the whole somewhat symmetrical.

Now, if we examine into the properties of this construction, it will be found that the material so distributed will sustain in the upper section (at only twenty tons to the square inch) a tensile strain of 4600 tons; and provided we deduct one-third for riveted cross joints, we still have upwards of 3000 tons for the strength of suspension, independent of the sides and bottom, which, if made of thick-edged plates, would give the power of resistance above 4000 tons.

In this hasty sketch I have not gone into the question of weights; but at a rough calculation, I should suppose one tube of, say 500 feet, would weigh about 500 tons, equal to a total of 3000 tons, in two lines of 1500

Fig. 2.



feet each. These views will not interfere with the experiments now in progress, but I should be glad of your opinion in order to guide me in the future development of principles which at present are but imperfectly understood. I am, my dear Sir, always sincerely yours,

Robert Stephenson, Esq., C.E., WILLIAM FAIRBAIRN.
&c. &c. &c.

From the very commencement of the inquiry, the means which should be employed in erecting and raising such a large structure over a broad and rapid stream, was a subject of deep reflection; and the point seemed indeed surrounded with impediments not easily overcome. It was probably not essential to discuss such a question, before the practicability of the scheme was ascertained, and the form and dimensions of the tube itself determined on; but it nevertheless weighed upon the mind, and, as will be seen by the letters, numerous expedients were originated and discussed before the means actually adopted were matured. My first thoughts were expressed in these terms to Mr. Stephenson:—

MY DEAR SIR, Manchester, June 3rd, 1845.

My mind has been fully engrossed with the subject of the Menai Bridge. Whatever may be the principle upon which it is to be constructed, there will be no difficulty in its erection, nor will there be any considerable expense incurred.

If, for example, the piers were erected and the whole of the tubes riveted together in sections, we could first commence the erection by fixing the centre parts upon the saddles of the piers; this being accomplished, we should then proceed in each direction, fixing the sectional parts, and maintaining the weights and balance equally on both sides, till they meet in the middle, thus:

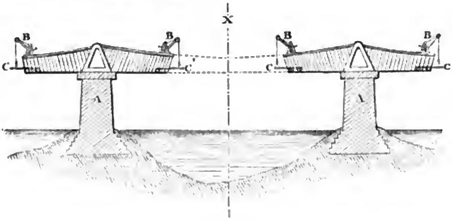


Fig. 3.

On this plan, the parts would always be in equilibrium on the middle of the piers, as at A A, and by means of two moveable cranes, and a working scaffold, as B B and C C, we could work progressively forward with 10 feet sections till they met at X, when the two could be united in the usual way with rivets.

By these means I think every difficulty with regard to erection would vanish; and, provided the parts were well put together in the first instance, we might carry such a structure across any straits or ravine of even greater extent than the Menai Straits. I shall be glad to hear your opinion in addition to my former letter upon this and other matter, and remain, My dear Sir, yours sincerely,

Robert Stephenson, Esq., C.E. WILLIAM FAIRBAIRN.

The above letter proposes a self-constructing system of erection, to avoid, if possible, the dangers and difficulties attending the moving and raising of such an enormous weight to a height of 100 or 120 feet, and then fixing it with safety upon the piers. On mature consideration, it appeared next to impossible to maintain the balance of so great a mass upon the pier as a fulcrum, and so to keep both ends in an exact line (as regards their horizontal and lateral position), as to cause them to meet in the middle. The plan was therefore abandoned for another of a more tangible kind.

Previous to the date of the following letter, July 19, considerable progress had been made with the experiments on the cylindrical tubes*, all of which were found more or less defective. Great difficulty was experienced in keeping them in shape; and the experiments, whilst they gave ground for confidence as to the ultimate result, showed pretty clearly that it was not to be attained under this form of tube. These facts are intimated to Mr. Stephenson as follows:—

MY DEAR SIR, Manchester, July 19th, 1845.

The last course of experiments was not only satisfactory, but such as to induce a considerable alteration in form, as well as a much more extensive investigation. These researches require the utmost care; and new developments, as well as correct results, are only obtained by repeated trials of an experimental and inductive character. It shall be my special province to ascertain the facts, and determine the law which governs the strength and form of this important structure.

---

* Vide Experiments A, B, &c. in the Appendix.

* * * * * * * * * *

Previous to leaving town, I gave orders for an increased number of tubular models to be made of different forms and dimensions. Towards the end of the ensuing week they will be completed, and I purpose again commencing the experiments on Wednesday morning, the 30th inst., when I hope you will be enabled, along with your father, to attend.

I am, my dear Sir, always sincerely yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

MY DEAR SIR, July 21st, 1845.

* * * * * * * * * *

I am glad to hear that you are proceeding with other forms of tubes; I hope some of them, of an elliptical form, and with thick plates at the top and bottom, will be tried, for in this way the disposal of the material will approach nearly to that of a common T girder, which is doubtless the thing to be aimed at.

Yours faithfully,

W. Fairbairn, Esq., ROBERT STEPHENSON.
Millwall, or Manchester.

At this time, July 19th—21st, a considerable number of experiments had been made: nearly the whole of the cylindrical tubes had been tested*, and preparations were then in progress for the rectangular and elliptical forms. The difficulties experienced in retaining the cylindrical tubes in shape, when submitted to severe strains, naturally suggested the rectangular form; many new models of this kind were prepared and experimented upon, before the end of July†; and others, with different thicknesses of the top and bottom plates or flanches, before the 6th of August‡. This is clearly indicated by the date of the experiments, and the letter of August 6th, addressed as before to Mr. Stephenson. This letter, it will be observed, strongly enforces the striking and *unmistakeable* evidence, afforded by the experiments, of the necessity of a close adherence to the principle of the simple beam or girder.

---

* See dates of the Experiments on the Cylindrical Tubes in the Appendix.

† Vide Appendix, Experiments on the Rectangular Tubes.

‡ The dates will be found in the Appendix.

Up to this period, my object had been to test the principle, originally suggested by Mr .Stephenson, of a structure, every part of which, although rigid, should be brought into a state of tension, and whose strength should consist, not as that of a beam or girder does, in its resistance to extension on the one side, and to compression on the other, but in a resistance to extension on both sides. For the adoption of such a form, if it could have been found, there was this plausible argument,—that the tenacity of wrought-iron being much greater than its resistance to compression, there would obviously have been an economy of the material, in so shaping the tube as to call into action its tenacity only. All my attempts to find such a shape as this were however fruitless. Every experiment gave the most certain evidence of a compression on the one side of the tube and extension on the other, and it yielded alike when the one resistance or the other was overcome. From this time the question presented itself therefore under a simpler form. I looked upon the tube as *a hollow girder,* whose strength was dependent upon the same causes as that of any other, and I saw plainly the direction my experiments should take and the principle by which I was to be guided. That determined opposition, which I shall be found in this correspondence to have given to the use of chains, or any other flexible auxiliary for the support of the tube, dates from this period.

My dear Sir, Millwall, August 6th, 1845.

For the last eight days, I have been constantly employed on the experiments, and although some of them have not always indicated the results expected, they are nevertheless not only useful, as regards the object of our research, but highly satisfactory.

From these investigations we derive several important facts, one of which I may mention, namely, the difficulty of bringing the upper, as well as the lower side of the bridge, into the tensile strain. For this object several changes were effected, and attempts made to distribute the forces equally, or in certain proportions throughout the parts, but without effect, the results being in every experiment that of a *hollow beam* or *girder*, resisting, in the usual way, by the compression of the upper and extension of the lower sides. In almost every instance we have found the resistance opposed to compression the weakest; the upper side generally giving way from the severity of the strain in that direction.

These facts are important so far as they have given rise to a new series of experiments, calculated to stiffen or render more rigid the upper part of the tube, as well as to equalize the strain, which in our present construction is evidently too great for the resisting forces of compression.

I entertained hopes of seeing you here before now, as I was anxious to show you the more interesting portion of the experiments, and to have had the benefit of your suggestions and advice.

As it is, and under present circumstances, I trust I have your permission to pursue the inquiry, and to introduce such new forms and combinations as will fully determine the law of resistance, and also the strongest form of tube, when acted upon by a force calculated to crush or tear it asunder.

I am leaving by this evening's train for Manchester, and will again return to the experiments in about a fortnight, or as soon as the additional tubes are prepared; in the mean time you will probably report progress, as some of the Directors and Secretary were here on Saturday for that purpose.

I am, my dear Sir, very faithfully yours,

Robert Stephenson, Esq., C.E. WILLIAM FAIRBAIRN.

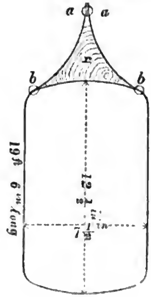
It will be seen by this letter, that the weakness of the tube had been recognized in its upper surface, which yielded to compression before the under side was upon the point of yielding to extension; and that the course which the experiments henceforth took of so strengthening the upper surface, that it should not be on the point of yielding to compression until the under surface was about to yield by extension, had been already shaped out. This state of the tube was a condition necessary to the greatest economy of its material, for in any state in which it was not on the point of yielding on the one side at the instant when it was on the point of yielding on the other, some of the material might be taken from the stronger side without causing that to yield, and added to the weaker so as to prevent that side from yielding, and thus the tube would be rendered stronger by a new distribution of its material. It was with a reference to this principle, that the rectangular form of section had suggested itself to me, in the place of the circular or the elliptical forms proposed by Mr. Stephenson, and that I had ordered the *top of the tube* to be thickened. It now occurred to me, that the top might be strengthened more effectually by other means than by thickening it, and I addressed the following letter to my son, four days after the date of the last, directing him to cause two additional tubes to be constructed, the one rectangular and the other elliptical, with hollow triangular cells or *fins* to prevent crushing.

These experiments led to the trial of the rectangular form of tube with a corrugated top, the superior strength of which decided me to adopt that cellular structure of the top of the tube which ultimately merged in a single row of rectangular cells. It is this cellular structure which gives to the bridges, now standing across the Conway Straits, their principal element of strength.

Manchester, August 10th, 1845.

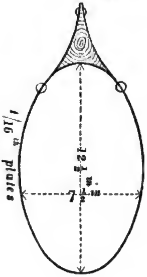
I shall require the following models made. One of this kind, composed of plates $\frac{1}{16}$th of an inch thick, the top, *a, a*, to be made of plates of the same thickness, but the middle part *x* may be left out, and filled up with wood to give the top side stiffness. The joints of the plates below to be carefully made with a stronger piece, double-riveted, over these, in order to cause the *plates* to be torn asunder, instead of the joint. The top, *a, a*, to be firmly riveted to the tube all the way along, as shown at *b, b*.

Fig. 4.



Another of the same kind to be made of the same length and thickness of plates, but of this shape, 19 ft. 6 in. long. These may be made out of some of the old ones, after proper drawings have been taken of them. To be crushed or torn asunder with the weights suspended from the inside as before.

Fig. 5.



Also one small beam 12 ft. long, as under:—

Thickness of top plate ¼th of an inch.
Sides $\frac{1}{16}$th of an inch.
Bottom ⅛th of an inch.

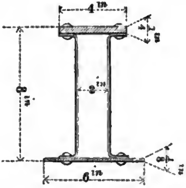
I think these will be all we shall require at present; and as soon as you are ready, let me know, and I will be with you to see them tested.

I am, yours affectionately,

William Fairbairn.

Thomas Fairbairn, Esq.

Fig. 6.



The experiments had now assumed a shape which seemed to me to require the assistance of a mathematician, who should deduce, if that were possible, a formula which, from the observed strength of a tube of a lesser, might enable me to calculate the strength of one of a greater size; and conceiving that Mr. Hodgkinson, now Professor at University College, London, would not object to undertake the discussion of such a formula, I applied to him to do so, and invited him to Millwall to witness some of the experiments then in progress.

Mr. Hodgkinson did not visit Millwall till the following month, being at that time engaged in testing some railway bars at the British Iron Works, South Wales, as is shown by the following extract from his letter.

British Iron Works, Abersychan near Pont-y-pool,
August 18th, 1845.

My dear Sir,

You expressed a wish that I should let you know when I left this place, and mentioned that you might wish me to go to London from here, instead of returning direct to Manchester.

I expect I shall be at liberty to leave here for the present this week end, and will place myself entirely at your disposal, either to go to one place or the other. * * * *

I am, my dear Sir, most truly yours,

William Fairbairn, Esq., C.E. Eaton Hodgkinson.

During Mr. Hodgkinson's first visit to Millwall (Sept. 19th, 1845), the whole of the experiments which had then been concluded were explained to him, and he carefully examined the apparatus which I had used. *On the first day of his visit, the tubes which had been constructed with single hollow or cellular tops were experimented on.* The forms of these tubes, and the results of the experiments upon them, are communicated to Mr. Stephenson in my letter of the 20th of September, which follows. One of them had a piece of fir timber fitted in the cell or fin, with a view to keep that part in form, and prevent it losing shape from the crushing forces, but in consequence of the difficulty which was experienced in making the timber accurately fill the whole space, it proved of little value. Superior results were however obtained from the increased surfaces which were offered to compression on the top sides by the fins, and my attention was naturally directed to the question, how far it was possible, by some other arrangement, better to accomplish the object of the cellular structure. Immediately upon the completion of the experiments on the "fin" tubes, I ordered the preparation of another form with a corrugated top, resembling in section the eyes of a pair of spectacles, rightly anticipating from this form of tube a considerable increase of strength. The cellular form of top offered, however, according to the experiments already made, such decided advantages, that in the following letter I ventured to anticipate, even at this early period, an ultimate cellular form of section for the great bridge itself, and proposed to Mr. Stephenson two ideas, the one of a tube with a series of square cells on the top, as at *a*, *a*[*] (see fig. 9), and the other with a number of circular pipes having flat plates riveted to their tops and bottoms, as shown at A (see fig. 10). The reader will not fail to observe how much the first of these sketches resembles the tubes actually constructed for the Conway and Britannia Bridges. The whole of the arrangements for our subsequent proceedings were pointed out at the time to Mr. Hodgkinson, and appeared to meet with his approval.

(*Private.*)

MY DEAR SIR, Millwall, Sept. 20th, 1845.

I have been uninterruptedly employed on the experiments for the whole of this week, and for two days I have had the benefit of the presence and assistance of my friend Mr. Hodgkinson. According to his views, as well as my own, we are progressing satisfactorily, and although we have not as yet arrived at the strongest form of tube, we are nevertheless approaching that desideratum. You will be aware, on referring to my last letter, that the great difficulty we had to encounter was a due proportion of the parts, so as to neutralize, or render the two resisting forces of compression and extension equal: out of nine experiments on cylindrical tubes, two failed by crushing in at the top, and seven by tearing asunder at the rivet-holes. The latter were however fractured, owing to the closeness of the rivet-holes and the construction of the tubes, the foreman having omitted to cross the joints.

From eleven experiments on rectangular tubes, eight yielded to the crushing force, and three only were torn asunder by extension.

The elliptical or egg-shaped tubes invariably failed, with only one exception, by compression; four having been crushed in at the top, and only one torn asunder at the bottom. Collectively, these appeared to indicate weakness on the upper side of the tube, and a necessity for a change of form in order to give rigidity and stiffness to that part. To counteract the forces of compression, I got two tubes constructed of the

[*] See sketches in letter.

annexed forms; one elliptical, with a deep fin *a* on the top, and the other rectangular, with a similar fin at *b*, as per annexed sectional sketch. These were, according to the dimensions here marked, the one 12 by 7½ inches, and the other 13 by 8 inches, and 18 feet 6 inches between the supports. The plates were $\frac{1}{16}$th of an inch thick, and the tubes broke or were crushed respectively with dead weights of 6867 and 8812 lbs.

Fig. 7.

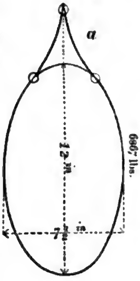
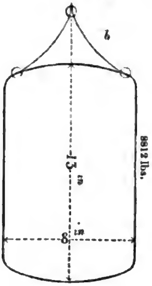
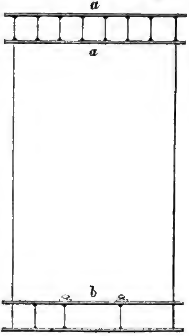


Fig 8.



The defective powers of resistance of all the tubes of this shape, have suggested a new arrangement and distribution of the metals; it being evident from the experiments, that the tube will resolve itself into a huge hollow beam or girder, leaving the two resisting forces of compression and extension as wide apart as possible. It is further conclusive, that the sides must be made comparatively light, and considerable additional material introduced into the top and bottom of the tube. This will give greatly increased strength, and a few more experiments will determine which of the two shall have the preponderance. It is more than probable that the bridge, in its full size, may take something of the following sectional shape.

Fig. 9.



The parts *a, a,* being two longitudinal plates, divided by vertical plates so as to form squares, calculated to resist the crushing strain in the first instance, and the lower parts, *b, b,* also longitudinal plates, well-connected with riveted joints, and of considerable thickness to resist the tensile strain in the second: or it may resolve itself into something of this form, (see the next fig.), with a series of tubes extending the whole length of the upper side, to resist compression, and two tubes with strong longitudinal plates, *c, c,* to resist tension, and prevent tearing in that part: the sides in this case to be made light, in order simply to connect the upper and lower resisting portions of the structure.

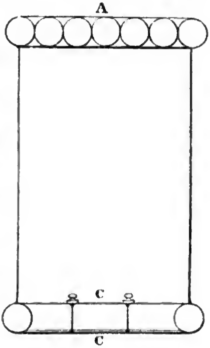
From the above statement, you will perceive that we are still short of the correct form. The experiments are progressively developing facts, which I have no doubt will enable me, with the assistance of such an able experimenter and excellent mathematician as my friend Mr. Hodgkinson, to lay before you such results, as will fully justify the adoption of this important structure. In the meanwhile I shall be glad to hear from you; and, requesting the favour of your opinion,

I am, my dear Sir, yours sincerely,

William Fairbairn.

Robert Stephenson, Esq., C.E.

Fig. 10.



I have written the word "private" at the commencement of this letter, as it is merely written off-hand, in order to put you in possession of the progress we are making in these researches, without being pledged to facts. I should think another week's investigation, which I expect will take place in a fortnight, will enable me to speak more definitely.

The following letter from Mr. Hodgkinson explains his views respecting the experiments which had been made, and also gives his ideas as to the form of tubes, and the experiments which he should recommend for trial. His suggestions were carried out, in accordance with the instructions sent to Millwall on October 2, but the experiments showed that the forms which he recommended were weak and unsatisfactory.

My dear Sir, Abersychan, September 26, 1845.

I have received your letter with the remainder of the experiments, but what to do with them I am quite at a loss. I have no principles to guide me, satisfactory to my own mind, nor the aid from books, which I should have if I was at home.

I have done my best to reduce some of the experiments; but the results are so much at variance that I am completely puzzled. What adds greatly to the difficulty, is their want of adaptation to mathematical requirements.

I shall not have it in my power to leave this place before the end of next week, without doing the parties a great injustice, but will do all I can on my return. I have been very unwell since my return from London, in consequence of the cold I caught there.

I mentioned to you before, that there are fundamental experiments necessary to make any useful application of these of yours; but while I am here I can do nothing, and before I should be at liberty to do anything, but try to reduce the experiments made, and suggest some modifications of them, it would be necessary to get the sanction of Mr. Stephenson, I suppose.

Were I at liberty to make any experiments, I would begin with some cylinders as free from rivets as possible near to the middle, so that they should break in a part not riveted. This might be done by taking long plates for the middle of the cylinder, and lengthening it out by small plates, since the rivets in them would not signify. I would have three cylinders made, all of the same diameter and length, say 18 or 20 inches diameter at the least, and the length 16 or 17 times the diameter. One of these cylinders should have its plates half an inch thick, and the others $\frac{1}{4}$th and $\frac{1}{8}$th of an inch. I would have them simple cylinders with one row of rivets on each side—none at the bottom or top near to the middle—nor any aperture made there for the shackle. A section of the cylinder would be as here represented, where I would rather there were no rivets; but as they cannot be avoided, they must be introduced. The length of the cylinder would be as below.

Fig. 11.

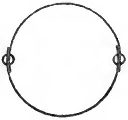
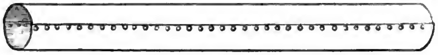


Fig. 12.



I would have three other cylinders made, of half the diameter and length as the preceding one; and their plates should be of half the thickness as those above, say $\frac{1}{4}$th, $\frac{1}{8}$th, $\frac{1}{16}$th of an inch; these cylinders should be riveted *on the side only*, as in the former case.

If you think well to prepare these for me, I will make the experiments on my return to Manchester; and the results will throw a little light upon the subject and enable me to judge better of the anomalies in your tube experiments. There appears to me, however, to be a great want of fundamental information on the subject.

I am, my dear Sir, yours most truly,

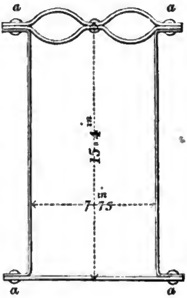
William Fairbairn, Esq. EATON HODGKINSON.

Postscript.—I am much in want of a book, "Navier's Application de Mécanique." I have thought of sending home for it; but perhaps by the time it arrived, I should be ready to return. I know there is something to my purpose in "Navier." You have the book likewise.

Mr. Hodgkinson did not experiment upon these tubes although they were constructed for him, since he found, from the experiments which had already been recorded, that the rectangular forms were decidedly the best; accordingly the models subsequently made for him were of this form. The interval of time which ensued, between the experiments on the rectangular and elliptical tubes with single cells, and those on the tube with a corrugated top, was chiefly occupied in preparing this latter tube for experiment. On the 14th of October it was subjected to the usual tests, as explained in the following letter addressed to Mr. Stephenson.

MY DEAR SIR, Manchester, Oct. 15, 1845.

Fig. 13.



Our experiments of yesterday were the best and most satisfactory we have yet made; and agreeable to expectation, the form, as per annexed sketch, gave not only the greatest strength, but what was of equal importance, there was a near approximation to an equality of the forces on the top and bottom sides.

The figure, you will observe, was in strict accordance with the views contained in my letter, which followed our previous efforts; the section being a rectangular figure, as above, with a double corrugated plate, each plate for the bottom being 0·180 inch. The sides were only 0·070 inch thick, and the whole riveted together, as at *a, a, a, a.*

With this form of tube (19 feet distant between the supports) the deflections were carefully taken at intervals, from weights of about 1800 lbs.; and before the tube gave way, which it did by tearing the sides from the top and bottom, it sustained for some minutes a weight of 22,469 lbs.

From this last experiment it is evident we are approaching the strongest form; but we are still in want of further investigation, in order to obtain a correct and satisfactory formula for the reduction of the experiments.

Some existing formulæ of "Navier" and others can be applied, but they are not satisfactory; and before my friend Mr. Hodgkinson can satisfy himself on the mathematical part of the case, some further experiments must be made on *exceedingly small* and *greatly enlarged* tubes, with certain functions, calculated to establish the law which governs, not only the strength of the present, but of all future forms of tubes.

This will however not create any delay, as I have ordered the plates, and I shall have the tube constructed forthwith, the experiments being made here in Manchester, on account of the more powerful apparatus which we have at command. In the mean time, I think we have sufficient data to guide you as to the security of such a structure; and provided you will fix time and place where we can meet, I shall lay before you such information as will, I trust, justify the measures you intended to adopt. Any day after next Tuesday I could meet you in London or elsewhere.

I am, my dear Sir, very sincerely yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

This altered form of the tube, as well as the improved distribution of the material, not only balanced the two resisting forces of extension and compression, but, what was of equal importance, appeared to establish the law which governs the strength of wrought-iron tubes, and thus enabled us to judge, with greater precision, as to the comparative values of these resisting forces.

In these experiments, the ratio between the areas of the top and bottom sides of the tube was nearly as 5 : 3; and although subsequent trials, upon a larger scale*, with better proportioned and more perfectly constructed cells, gave improved results, these experiments were nevertheless highly satisfactory and supplied sufficient data on which to proceed, with some degree of certainty, in preparing the designs and sectional drawings for the large tubes.

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* Vide experiments on the model tube in the Appendix.

It is from this period that I date the disappearance of almost every difficulty respecting the construction and ultimate formation of the Britannia and Conway tubes. The powerful resistance offered to compression by the cellular form of the top, as exhibited in the last experiment, at once decided, in my mind, the form to be adopted in those for the large tubes, and from this time forward I had no doubts as to the practicability and complete success of the undertaking. If the tube had been already constructed, I could not have seen my way more clearly than I did on this occasion, and my only regret was, that Mr. Stephenson did not entertain the same degree of confidence.

My mind was so strongly impressed with the necessity of perseverance, that I never lost sight of the great object of our experiments, till the Conway tube was completed and safely landed in its place. I never thought of failure—I had no misgivings as to the result. The conviction on my mind was, in fact, so strong as to dissipate every fear, and this was accompanied with a determination to encounter every difficulty and to overcome it. It was owing to this feeling of confidence, that I combated every opposition, gave offence to some of my friends, and ultimately carried Mr. Stephenson along with me to the completion of the bridge. Indeed, that gentleman will not disclaim his own repeated declarations, "that my unlimited confidence, and determined powers of resistance to those who condemned the proceedings, tended, almost more than anything else, to build the bridge."

In the foregoing letter, it will be noticed, that, in speaking of the experiment, I observe, "we are still in want of further investigation, in order to obtain a correct and satisfactory formula for the reduction of the experiments." It will also be observed, that the experiments which had already been made did not appear satisfactory to Mr. Hodgkinson; but in order that no ambiguity should rest upon them, it was decided that he should make his own experiments, and from them, if possible, deduce formulæ better adapted to the calculations then in progress.

However I must state, that Mr. Hodgkinson's expected formulæ did not appear in time to be of any service in proportioning the parts of the large tubes. The designs and drawings for the tubes of both bridges were now in progress, and were too far advanced to admit of any *essential* change. In fact, they were designed and proportioned from the experiments made on the tube with the corrugated top, and from these experiments were deduced certain formulæ*, by which were computed the strengths and other proportions of the Britannia and Conway tubes.

These calculations and formulæ however were afterwards revised, by a more extended scale of experiments made upon the model tube, which gave better proportions, and caused some modifications to be made in the areas of the top and bottom sides, which were now taken nearly in the proportion of 6 to 5, in the place of 5 to 3, as indicated by the previous experiments.

The next letter is from Mr. Stephenson, approving of the whole of the proceedings, and expressing himself perfectly satisfied with the results derived from the experiments.

Leeds, Thursday.

MY DEAR SIR, (Without date, about the end of October, 1845.)

I am really ashamed to take up my pen to write to you. My silence must have given you the idea, that I am perfectly indifferent about the interesting series of experiments you have been so kind as to undertake. The vast and gigantic character of the object we have in view, will, however, I trust, protect me from this suspicion.

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* The drawings and designs for the Britannia and Conway Bridges were made out, and the parts proportioned, without the aid of Mr. Hodgkinson's formulæ; and the above, as well as other hollow girder bridges, have since been constructed independently of that gentleman's assistance.

Your last letter, reporting progress, was extremely satisfactory, and I was delighted to hear that you had succeeded in obtaining the assistance of Hodgkinson. I hope sincerely he will continue to aid us in our investigations. I purpose being in London all next week, when I should be glad to meet you at Millwall. It now becomes a matter of necessity that I should spend some time with you, because I am compelled to leave for Italy towards the end of the month, and before my departure we must decide upon some course for the two succeeding months.

I am pretty well decided as to the masonry, which should be very shortly commenced, but I must have some discussion with you beforehand.

Yours faithfully,

William Fairbairn, Esq., Engineer, Manchester. ROBERT STEPHENSON.

From the month of October till nearly the end of November Mr. Stephenson was professionally engaged abroad, and, during that time, little or no progress was made in the experiments. Several letters however were written, and numerous suggestions made, relative to the construction and form of the cells and other parts of the tubes. Among others was that of December the 3rd, wherein the form of preparing the report is stated*, and also the shape of another experimental tube, which was suggested for the purpose of rendering the experiments previously made still more conclusive and satisfactory.

The time occupied in Mr. Stephenson's journey to Italy afforded opportunities for consideration, as to the best attainable means for the connexion and form of the material to be used in the construction of the large tubes. Many suggestions presented themselves, and numerous sketches and drawings were made with reference chiefly to the details of the construction. One of the principal practical considerations in the construction, was to provide a convenient mode of approach to all parts of the structure, for the purpose of cleaning, painting, &c. This was regarded essential by both Mr. Stephenson and myself, and hence follow the suggestions, as shown in the diagram and sectional sketch at *b*, *b*, see fig. 15. This plan was subsequently abandoned for a cellular top of a more simple form, nearly, if not equally, as effective in its powers to resist strain, and in all respects similar to that suggested to Mr. Stephenson, in my letter dated 20th September 1845.

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* The Directors up to this time evinced a great deal of patience, and watched with considerable interest the progress of the experiments made at Millwall. They however became anxious about the report, in order that they might have something definite to lay before the shareholders at their next general meeting.

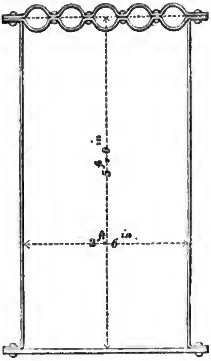
MY DEAR SIR, Manchester, December 3rd, 1845.

Since your departure, I have been chiefly employed in devising means for completing the experiments and preparing the report. I think you expressed a wish, that this document should be a joint affair, and bear the signature of all three, that is, of R. Stephenson, E. Hodgkinson, and myself. If such still be your wish, it can be so prepared; but in case you should prefer it, Mr. Hodgkinson could make out a separate statement, giving the details upon which the formula is founded; I would then give the experiments at length, and place the whole in your hands, to be inserted in the general report, which you would probably prefer drawing up in your own name. Will you oblige me with a letter, at your early convenience, on these points, as it will save time, and enable us to have the description and calculations in a state of forwardness, by the time of your return to this country? I think this arrangement would not only save time, but probably some trouble also.

In order to render the experiments conclusive and satisfactory, I have found it necessary to extend them considerably: first, to meet the mathematics of the case, and establish the law which governs the strengths; and secondly, to enlarge our views still further upon the subject, by another experiment upon the rectangular form, which being made, a certain proportion of those already experimented upon would enable us to draw conclusions with still greater certainty as to comparative results. For these objects I am now constructing a tube of about 73 feet 6 inches span, and of the following sectional dimensions:

The top to be composed of double corrugated plates, each $\frac{1}{16}$th of an inch thick, the sides $\frac{1}{16}$th of an inch, and the bottom $\frac{5}{16}$ths of an inch thick. These dimensions are, as nearly as possible, $\frac{1}{6}$th of the size and dimensions of the bridge; and judging from the last experiment, I should infer from the position of the material in the top and bottom, that the resistances of compression and extension would be nearly in equilibrium with each other. With this experiment, and those for Hodgkinson, I think, we shall be in a position (having so many facts before us) to state with considerable accuracy, what can be done, on the enlarged scale, six times the size of the tube we are now constructing.

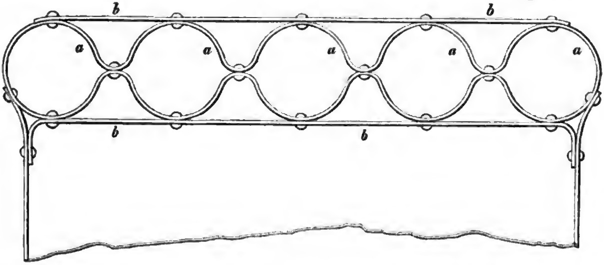
Fig. 14.



Section of model tube.

In constructing the bridge upon the large scale (assuming that the rectangular form, with a corrugated or tubular top, is the best calculated to resist compression), I would advise that the top part be constructed as per annexed sketch.

Fig. 15.



Upon this plan, we should obtain considerable tenacity and stiffness, with the least possible weight of material; and what is probably of equal importance, all the parts would be put together in a form adapted for the purpose of repairs, painting or construction, as the case may require. If the corrugated parts, *a, a, a, a, a,* 3 feet in diameter, were composed of $\frac{3}{8}$ths of an inch plates, and the straight plates, *b, b, b, b,* $\frac{5}{16}$ths of an inch, I have reason to believe we should obtain a degree of stiffness sufficient to carry 1000 tons on a span of 450 feet. You will please to ruminate over these matters, which I only offer as suggestions, and believe me always,

My dear Sir, yours faithfully,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

The two following letters, dated January 8th and 12th, 1846, contain confirmations of the proceedings and suggestions detailed in previous communications, and also an approval of the proposed arrangements for the report to the Directors. The next letter, dated January 13th, relates to the difficulties experienced by Mr. Hodgkinson, in his attempts to account mathematically for the "puckering" of the plates on the upper side of the tube. There was some hesitation on the part of Mr. Hodgkinson, relative to the manner and form in which the reports should appear. Ultimately, however, it was decided, that they should be given separately, in order to afford an opportunity for each reporter to give his own views in the statements to be laid before the Directors.

24 Great George Street, Westminster,
January 8th, 1846.

MY DEAR SIR,

Your letter missed me when I was abroad, but I have now received it, and I shall be glad to see you when convenient. I will come to Manchester if you like at once, as the Directors are pressing me to have a full report ready for them by the end of this month, in order that they may say something specific in reference to the bridge at the next general meeting, which will take place about the 6th or 7th of next month. I hope Mr. Hodgkinson has succeeded in obtaining a satisfactory formula. Hoping to hear from you by return,

I am, yours faithfully,

W. Fairbairn, Esq., Engineer, Manchester. ROBERT STEPHENSON.

MY DEAR SIR, Westminster, January 12th, 1846.

The mode suggested by you in your letter as to the report, I quite fall into. If you will report on the experiments, Mr. Hodgkinson on his mathematical deductions, I will upon these make a brief report to the Board.

The three reports will then form a complete document for the Directors. Pray do let us be prepared by the end of the month, for I cannot keep the Directors quiet any longer. I will come down to see you if possible.

Yours faithfully,

W. Fairbairn, Esq. ROBERT STEPHENSON.

MY DEAR SIR, Manchester, January 13th, 1846.

The report, accompanied with the experiments, will be arranged and drawn up as you request; namely, in three divisions, and we shall make an effort to have all prepared by the time you mention.

Both Mr. Hodgkinson and myself are a good deal puzzled with the flexible nature of the material; and although my last experiments at Millwall gave the most satisfactory results in the approximate value of the forces on each side of the neutral axis, there still exists considerable difficulty in accounting *theoretically* for the "puckering," and doubling up of the tube. We are both of us labouring hard to get over this difficulty, and hope shortly to reduce it to some fixed law. Meanwhile our experiments are not complete, until that important point is fully and satisfactorily ascertained.

I think you had better not visit Manchester for a few days, as I purpose being in London tomorrow night, and will call upon you at ten on Thursday morning.

Meanwhile, I am, yours faithfully,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

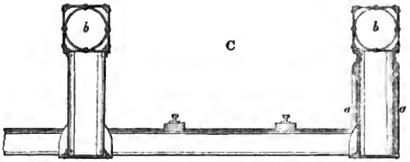
MY DEAR SIR, Manchester, February 4th, 1846.

According to promise, I beg to hand you the accompanying statement of experimental facts, connected with the proposed bridge across the Menai Straits. I trust you will find it in that form best calculated for the purpose of communicating with the Directors, and such as you will yourself approve. After the meeting, you will perhaps inform me as to your own intentions, and the opinion of the Directors. Mr. Hodgkinson and myself will continue to pursue our investigations, and I trust we shall not only have your sanction, but the support of the Directors in every attempt to bring the matter to a prosperous issue.

Since your departure yesterday, I have been thinking about your bridge across the Dee. Would not the subject, of which I spoke to Mr. Bateman, of three hollow sheet-iron girders answer better than anything else? I could wish you to try it, and should prefer you doing it, before even my own son-in-law. What I would propose, would be three hollow beams, each of them $\frac{1}{13}$th of the width of the span, and composed of plates as per annexed section.

From the experiments already made, it will be easy to determine the thickness of the plates requisite for bridges of this kind, averaging from 60 to 120 feet span. I think, they will be strong and cheap, and provided we attach a line of hollow plates, *a*, *a* in the form of an arch, well-riveted on each side, we may reasonably count on the required amount of rigidity and strength.

Fig. 16.



Perhaps these suggestions may be worthy of attention, and waiting your further instructions,

I am, my dear Sir, yours faithfully,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

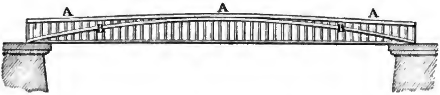
The preceding letter, which was forwarded to Mr. Stephenson along with my report to the Directors of the Chester and Holyhead Railway, was written at the time when the masonry was in progress for a girder bridge of the usual construction over the Dee at Chester, and upwards of sixteen months previous to its completion.

The suggestions, relative to the sheet iron girders, given in the above letter, were previously communicated to my son-in-law Mr. Bateman, and he made out drawings founded on these suggestions, which were shown to Mr. Stephenson, and recommended to him as being a form of girder especially applicable to bridges of great span. Arrangements having probably been already entered into for the employment of cast-iron trussed girders, my proposal, involving an entirely new principle of girder, was not adopted.

The sectional sketch represented at C (fig. 16.) was the only drawing contained in my letter to Mr. Stephenson. The complete drawings prepared by Mr. Bateman, exhibited a rectangular hollow beam, having at the top a cylindrical tube enclosed in a square of riveted plates, as shown in the sketch at *b*, *b*.

In addition to this form of cellular top, it was proposed to rivet an arch of semicircular hollow plates on the sides, to give rigidity to those parts, as shown in the annexed figure.

Fig. 17.



The parts A, A represent the cellular top, and B, B the proposed hollow arch riveted to the sides. These modes of obtaining additional strength were ultimately abandoned, for the simple square cells and straight sides, with interior T iron covering the vertical joints of the plates, which was found quite sufficient to give the requisite stiffness to the sides.

It may not be inexpedient here to insert the Reports, as given to the Directors of the Chester and Holyhead Railway, and read at the general meeting of the subscribers in February 1846. In these reports, will be seen the respective views of the reporters relative to the experiments, as well as to the chances of ultimate success in the construction of the Tubular Bridge across the Menai Straits. Mr. Stephenson states in his report, (which is evidently drawn up with great care) that he is satisfied with the results of my experiments, and as a necessary consequence, his opinion, as to the advantages of a simple tubular structure, appears to be considerably strengthened; he observes, "that the adoption of a wrought iron tube is the most efficient, as well as the most economical, description of structure that can be devised for a railway bridge across the Menai Straits."

Mr. Stephenson then goes on to explain the nature of the experiments, admitting the anomalous character of the results of some and the efficiency of others, and concludes this part of his report by stating, that "another instructive lesson which the experiments have disclosed is, that the rectangular tube is by far the strongest, and that the circular and elliptical should be discarded altogether."

Mr. Stephenson next takes a view of the formula obtained by Mr. Hodgkinson for the large rectangular tube, which indicates a breaking weight of 1100 tons, when the plates are assumed to be 1 inch thick. Now these calculations, however interesting in a scientific point of view, were of no use as regards the construction of the Menai Bridge; inasmuch as the rectangular tubes, which I had proposed, having a far more economical distribution of the material, are considerably stronger than those to which Mr. Hodgkinson's formula applies.

From the theoretical investigation of the ultimate strength of tubes, Mr. Stephenson proceeds at once to discuss the merits of my report, wherein it is stated, that the tube "should be a huge sheet-iron hollow girder, of sufficient strength and stiffness to sustain those weights (about 2000 tons); *and, provided the parts are well-proportioned, and the plates properly riveted, you may strip off the chains, and leave it as a useful monument of the enterprise and energy of the age in which it was constructed.*" To these views Mr. Stephenson does not appear fully to subscribe, nor did he do so for some months afterwards. At the close of his report, he merely adduces some examples of the danger attending the use of chains, in the ordinary form of suspension bridges, and concludes with a cautious reservation of opinion as to the adoption or rejection of such auxiliary means of support.

In my report, after giving a detailed account of the experiments which I had made, stating the results as well as pointing out the anomalous circumstances which occasionally presented themselves, I observe, "So far as our knowledge extends,—and judging from the experiments already completed,—I would venture to state that a Tubular Bridge can be constructed, of such powers and dimensions, as will meet, with perfect security, the requirements of railway traffic across the Straits. The utmost care must, however, be observed in the construction, and probably a much greater quantity of material may be required than was originally contemplated, before the structure can be considered safe."

Mr. Hodgkinson's report comprises much useful scientific information. He applies his formulæ to the finding of the amount of strain, per square inch, on certain simple geometrical forms of tubes; but it should be observed, that these calculations present no rule for estimating the strength of the material in tubes, having the cellular structure recommended by me. The decision come to by Mr. Hodgkinson is not in accordance with my views; for he concludes his report with observing, "If it be determined to erect a bridge of tubes, I would beg to recommend that suspension chains be employed as an auxiliary, otherwise great thickness of metal would be required to produce adequate stiffness and strength."

From a perusal of these reports, it will be seen, that neither Mr. Stephenson nor Mr. Hodgkinson entertained the idea of making the tubes support themselves, or of entirely dispensing with the dangerous incumbrances of chains. On the contrary, that form of structure (the form which the Menai Bridge now has) was advocated by me alone, in opposition to the views which these gentlemen had at that time, and which they from the very first had entertained, and to which they, for some months afterwards, tenaciously adhered. My objections to the employment of auxiliary chains, as I have already stated, were grounded on their flexible nature, and the antagonism of this quality to the perfectly rigid character of the tubular portion of the structure. That the chains have been stripped off the tubes, is therefore, in my opinion, one of the most important of the precautions which have been taken for the security of the bridge.

MR. STEPHENSON'S REPORT.

24 Great George Street, Westminster, Feb. 9, 1846.

To the Directors of the Chester and Holyhead Railway.

GENTLEMEN,

In reporting to you the progress which has been made in the works, I beg to refer you to the statements of Mr. Ross and Mr. Foster, made from time to time, as regards those under contract. In addition, I need only state, that last week I examined them personally, and found the whole progressing in the most satisfactory manner. I will therefore proceed at once to lay before you the results of the experimental investigation which, with your sanction, I commenced some months ago in reference to the construction of the bridge over the Menai Straits.

The object of this investigation, as you are aware, was to test the truth of the views I entertained respecting the employment of a large wrought iron tube, instead of cast iron arches, as was originally proposed, but which we were compelled to abandon, in consequence of the Admiralty refusing to allow the erection of such a structure, from the belief that it would injuriously interfere with the navigation of the Straits.

In conducting this experimental investigation, I saw the importance of avoiding the influence of any preconceived views of my own, or at least to check them, by calling in the aid of other parties thoroughly conversant with such researches. For this purpose, I have availed myself of the assistance of Mr. Fairbairn and Mr. Hodgkinson; the former, so well known for his thorough practical knowledge in such matters; and the latter, distinguished as the first scientific authority on the strength of iron beams.

These gentlemen have pursued the subject with deep interest; and although they have not yet been able to bring the facts into a final and definite shape, they have each complied with my request that they would communicate their views upon the results which have already been arrived at. I therefore append to this Report, their observations just as I received them. They will, I am confident, prove satisfactory to you.

I have, throughout the experiments, carefully studied the results as they developed themselves, and I am satisfied that the views I ventured to express twelve months ago, were in the main correct; and that the adoption of a wrought iron tube is the most efficient, as well as the most economical, description of structure that can be devised for a railway bridge across the Menai Straits.

In the course of the experiments, it is true, some unexpected and anomalous results presented themselves; but none of them tended in my mind to show that the tubular form was not the very best for obtaining a rigid roadway for a railroad over a span of 450 feet, which is the absolute requirement for a bridge over the Menai Straits.

The first series of experiments was made with plain circular tubes, the second with elliptical, and the third with rectangular. In the whole of these, this remarkable and unexpected fact was brought to light, viz. that in such tubes the power of wrought iron to resist compression was much less than its power to resist tension,—being exactly the reverse of that which holds with cast iron: for example, in cast iron beams for sustaining weight, the proper form is to dispose of the greater portion of the material at the bottom side of the beam,—whereas with wrought iron, these experiments demonstrate beyond any doubt that the greater portion of the material should be distributed on the upper side of the beam. We have arrived therefore at a fact having a most important bearing upon the construction of the tube; viz. that rigidity and strength are best obtained by throwing the greatest thickness of material into the upper side.

Another instructive lesson which the experiments have disclosed is, that the rectangular tube is by far the strongest, and that the circular and elliptical should be discarded altogether.

This result is extremely fortunate, as it greatly facilitates the mechanical arrangements for not merely the construction, but the permanent maintenance of the bridge.

We may now therefore consider that two essential points have been finally determined; the form of the tube, and the distribution of the material.

The only important question remaining to be determined, is the absolute ultimate strength of a tube of any given dimensions. This is of course approximately solved by the experiments already completed; but Mr. Hodgkinson very properly states, that others, with tubes of more varied dimensions, should be continued in order to clear up some anomalies which still exist.

The formula as at present brought out by Mr. Hodgkinson, gives the strength of a rectangular tube of the dimensions I proposed, viz. 450

feet long, 15 feet wide, by 30 feet high (assuming the plates to be 1 inch thick), equal to 1100 tons applied in the centre, including the weight of the tube itself; but, deducting the latter, equal to 747 tons in the centre, —or double this, supposing the weight to be uniformly distributed over the whole 450 feet.

This amount of strength, although sufficient to carry any weight that can in practice be placed upon the bridge, is not sufficiently in excess for practical purposes. It is on this ground, therefore, I have requested Mr. Hodgkinson to devise a few more experiments in the shape best calculated to free the formula from all ambiguity. In the meantime, however, as I consider the main question settled, I am proceeding with the designs and working plans for the whole of the masonry, which I expect to have the pleasure of submitting to you in a fortnight from this time.

You will observe in Mr. Fairbairn's remarks, that he contemplates the feasibility of stripping the tube entirely of all the chains that may be required in the erection of the bridge; whereas, on the other hand, Mr. Hodgkinson thinks the chains will be an essential, or at all events a useful auxiliary, to give the tube the requisite strength and rigidity. This, however, will be determined by the proposed additional experiments, and does not interfere with the construction of the masonry, which is designed so as to admit of the tube, with or without the chains.

The application of chains as an auxiliary has occupied much of my attention, and I am satisfied that the ordinary mode of applying them to suspension bridges is wholly inadmissible in the present instance; if, therefore, it be hereafter found necessary or desirable to employ them in conjunction with the tube, another mode of applying them must be devised, as it is absolutely essential to attach them in such a manner as to preclude the possibility of the smallest oscillation.

In the accomplishment of this I see no difficulty whatever; and the designs have been arranged accordingly, in order to avoid any further delay.

The injurious consequences attending the ordinary mode of employing chains in suspension bridges, was brought under my observation in a very striking manner on the Stockton and Darlington Railway, where I was called upon to erect a new bridge for carrying the railway across the River Tees, in lieu of an ordinary suspension bridge, which had proved an entire failure.

Immediately on opening the suspension bridge for railway traffic, the undulations into which the roadway was thrown, by the inevitable unequal distribution of the weight of the train upon it, were such as to threaten the instant downfall of the whole structure.

These dangerous undulations were most materially aggravated by the chain itself, for this obvious reason,—that the platform or roadway, which was constructed with ordinary trussing for the purpose of rendering it comparatively rigid, was suspended to the chain, which was perfectly flexible, all the parts of the latter being in equilibrium. The structure was, therefore, composed of two parts, the stability of the one being totally incompatible with that of the other: for example, the moment an unequal distribution of weight upon the roadway took place by the passage of a train, the curve of the chain altered, one portion descending at the point immediately above the greatest weight, and consequently causing some other portion to ascend in a corresponding degree, which necessarily raised the platform with it, and augmented the undulation.

So seriously was this defect found to operate, that immediate steps were taken to support the platform underneath by ordinary trussing; in short, by the erection of a complete wooden bridge, which took off a large proportion of the strain upon the chains. If the chains had been wholly removed, the substructure would have been more effective; but as they were allowed to remain, with the view of assisting, they still partake of these changes in the form of the curve, consequent upon the unequal distribution of the weight, and eventually destroyed all the connections of the wooden framework underneath the platform, and even loosened and suspended many of the piles upon which the framework rested, and to which it was attached.

The study of these and other circumstances connected with the Stockton Bridge, lead me to reject all idea of deriving aid from chains employed in the ordinary manner.

I have therefore turned my attention to other modes of employing them in conjunction with the wrought iron tube (as suggested by Mr. Hodgkinson), if such should be found necessary upon further investigation.

As I have already stated, in this I perceive no difficulty whatever; indeed, there is no other construction which has occurred to me, which presents such facilities as the rectangular tube for such a combination.

Having, I trust, clearly explained my views in reference to this important work, I have only to add, that in two months I expect every arrangement will be completed for commencing the masonry, which shall be conducted with the utmost activity and vigour.

I can scarcely venture to say, until after these arrangements are finally completed, at what period we may calculate upon the completion of this bridge: but I cannot recommend you to calculate upon the whole being accomplished in less than two years and a half.

I am, Gentlemen, your obedient Servant,

ROBERT STEPHENSON.

MR. FAIRBAIRN'S REPORT.

Abstract or short Summary of Results from Experiments relative to the proposed Bridge across the Menai Straits, addressed to Robert Stephenson, Esq. By W. FAIRBAIRN.

After a series of experiments undertaken at your request, for ascertaining the strongest form of a Sheet-Iron Tubular Bridge across the Menai Straits, I have been induced, in order to meet the requirements for such a structure, and to ensure safety in the construction, to call in the aid and assistance of my friend Mr. Hodgkinson.

The flexible nature of the material, and the difficulties which presented themselves in retaining the lighter description of tubes in shape, gave exceedingly anomalous results; and having no formula on which dependence could be placed for the reduction of the experiments, I deemed it necessary, in a subject of such importance, to secure the cooperation of the first authority, in order to give confidence to the Chester and Holyhead Railway Company, with whom you are connected, and the public generally.

It will be observed, that the first class of experiments are upon cylindrical tubes; the second upon those of the elliptical form; and the last upon the rectangular kind. Tubes of each sort have been carefully tested, and the results recorded in the order in which they were made; and moreover, each specimen had direct reference to the intended bridge, both as regards the length and thickness, as also the depth and width.

In the first class of experiments, which are those of the cylindrical form, the results are as follow :—

Cylindrical Tubes.

No. of experiments.	Distance between the supports.	Diameter in inches.	Thickness of plate in inches.	Ultimate deflection in inches.	Breaking weight in lbs.	Remarks.
	ft. in.					
1	17 0	12·18	·0408	·39	3,040	Crushed top.
2	17 0	12·00	·0370	·65	2,704	ditto.
3	15 7½	12·40	·1310	1·29	11,440	Torn asunder at the bottom.
4	23 5	18·26	·0582	·56	6,400	ditto.
5	23 5	17·68	·0631	·74	6,400	ditto.
6	23 5	18·18	·1190	1·19	14,240	ditto.
7	31 3¼	24·00	·0954	·63	9,760	ditto.
8	31 3¼	24·30	·13501	·95	14,240	ditto.
9	31 3¼	24·20	·0954	·74	10,880	ditto.

With the exception of the first two, nearly the whole of the tubes were ruptured by tearing asunder at the bottom through the line of the rivets.

Finding the cylindrical form comparatively weak, the next experiments were upon tubes of the rectangular shape, which gave much better results. For the present, it may, however, be more convenient to take the elliptical kind, as being the nearest approximation, as regards both form and strength, to the cylinders recorded above.

Elliptical Tubes.

No. of experiments.	Distance between the supports.	Diameters, transverse and conjugate, in inches.	Thickness of plates in inches.	Ultimate deflection in inches.	Breaking weight in lbs.	Remarks.
	ft. in.					
19	17 0	14·62 9·25	·0416	·62	2,100	Crushed on top.
20	24 0	21·66 13·50	1·1320	1·36	17,076	Broke by extension.
21	24 0	21·25 14·12	·0688	·45	7,270	By compression.
22	18 6	12·00 7·50	·0775	·95	6,867	By compression. This tube had a fin on the top side.
24	17 6	15·00 9·75	·1430	1·39	15,000	Both sides were ruptured.

It will be observed that the whole of these experiments indicated weakness on the top side of the tube, which, in almost every case, was greatly distorted by the force of compression acting in that direction. It is probable that those of the cylindrical form would have yielded in like manner, had the riveting at the joints been equally perfect on the lower side of the tube. This was not, however, the case, and hence arise the causes of rupture at that part.

The next experiments, and probably the most important, were those of the rectangular kind; they indicate a considerably increased strength when compared with the cylindrical and elliptical forms; and, considering the many advantages which they possess over every other yet experimented upon, I am inclined to think them not only the strongest but the best-adapted (either as regards lightness or security) for the proposed bridge.

Rectangular Tubes.

No. of experiments.	Distance between supports.	Depth in inches.	Width in inches.	Thickness of plate in inches.		Ultimate deflection in inches.	Breaking weight in lbs.	Remarks.
				Top.	Bottom.			
	ft. in.							
14	17 6	9·6	9·6	·075	·075	1·10	3,738	Broke by compression.
14	17 6	9·6	9·6	·272	·075	1·13	8,273	(Reversed). Extension.
15	17 6	9·6	9·6	·075	·142	0·94	3,788	Compression.
15*a*	17 6	9·6	9·6	·142	·075	1·88	7,148	Extension.
16	17 6	18·25	9·25	·059	·149	0·93	6,812	Compression.
16*a*	17 6	18·25	9·25	·149	·059	1·73	12,188	ditto.
17	24 0	15·00	2·25	·160	·160	2·66	17,600	ditto.
18[*]	18 0	13·25	7·50	·142	·142	1·71	13,680	ditto.
23	18 6	13·00	8·00	·066	·066	1·19	8,812	Compression. Circular bottom, fin at top.
29	19 0	15·40	7·75	·230	·180	1·59	22,469	Sides distorted. Corrugated top.

On consulting the above table, it will be found that the results as respects strength are of a higher order than those obtained from the cylindrical and elliptical tubes; and particularly those constructed with stronger plates on the top side, which, in almost every experiment where the thin side was uppermost, gave signs of weakness in that part. Some curious and interesting phenomena presented themselves in these experiments,—many of them are anomalous to our preconceived notions of the strength of materials, and totally different to anything yet exhibited in any previous research. It has invariably been observed, that in almost every experiment the tubes gave evidence of weakness in their powers of resistance on the top side, to the forces tending to crush them. This was strongly exemplified in experiments 14, 15, 16, &c. marked on the drawings and the table. With tubes of a rectangular shape, having the top side about double the thickness of the bottom, and the sides only

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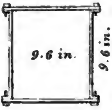
[*] Nearly $\frac{1}{16}$th the height.



half the thickness of the bottom, or one-fourth the thickness of the top, nearly double the strength was obtained. In experiment 14 (marked in the margin of the above table) a tube of the rectangular form, as per annexed sketch, 9½ inches square, with top and bottom plates of equal thickness, the breaking weight was . . . . 3738 lbs.

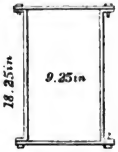
Riveting a stronger plate on the top side, the strength was increased to . . . . } 8273 lbs.

The difference being . . . . 4535 lbs., considerably more than double the strength sustained by the tube when the top and bottom sides were equal.

The experiments given in No. 15 are of the same character, where the top plate is as near as possible double the thickness of the bottom. In these experiments, the tube was first crippled by doubling up the thin plate on the top side, which was done with a weight of . . . . . 3788 lbs.

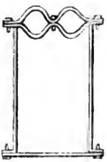
It was then reversed with the thick side upwards, and by this change the breaking weight was increased to . . . } 7148 lbs.

Making a difference of . . . . 3360 lbs., or an increase of nearly double the strength, by the simple operation of reversing the tube, and turning it upside down.



The same degree of importance is attached to a similar form, when the depth in the middle is double the width of the tube. From the experiments in No. 16, we deduce the same results in a tube of the annexed sectional form, where the depth is 18¼, and the breadth 9¼ inches.

Loading this tube with 6812 lbs. (the thin plate being uppermost), it follows precisely the same law as before, and becomes wrinkled, with a hummock rising on the top side so as to render it no longer safe to sustain the load. Take, however, the same tube, and reverse it with the thick plate upwards, and you not only straighten the part previously injured, but you increase the resisting powers from 6812 lbs. to 12,188 lbs. Let us now examine the tube in the 29th experiment, where the top is composed of corrugated iron, as per sketch, forming two tubular cavities extending longitudinally along its upper side. This, it will be observed, presents the best form for resisting the "puckering," or crushing force, which, on almost every occasion, was present in the previous ex-



periments. Having loaded the tube with increasing weights, it ultimately gave way by tearing the sides from the top and bottom plates, at nearly one and the same instant after the last weight, 22,469 lbs., was laid on. The greatly increased strength indicated by this form of tube is highly satisfactory, and provided these facts be duly appreciated in the construction of the bridge, they will, I have no doubt, lead to the balance of the two resisting forces of tension and compression.

The results here obtained are so essential to this inquiry, and to our knowledge of the strength of materials in general, that I have deemed it essential, in this abridged statement, to direct attention to facts of immense value in the proper and judicious application, as well as distribution, of the material in the proposed structure. Strength and lightness are desiderata of great importance,—and the circumstances above stated are well-worthy the attention of the mathematician and engineer.

For the present we shall have to consider not only the due and perfect proportion of the top and bottom sides of the tube, but also the stiffening of the sides with those parts, in order to effect the required rigidity for retaining the whole in shape. These are considerations which require attention: and till further experiments are made, and probably some of them upon a larger scale, it would be hazardous to pronounce anything definite as to the proportion of the parts, and the equalization of the forces tending to the derangement of the structure.

So far as our knowledge extends,—and judging from the experiments already completed,—I would venture to state that a Tubular Bridge can be constructed, of such powers and dimensions as will meet, with perfect security, the requirements of railway traffic across the Straits. The utmost care must, however, be observed in the construction, and probably a much greater quantity of material may be required than was originally contemplated before the structure can be considered safe.

In this opinion Mr. Hodgkinson and myself seem to agree: and although suspension chains may be useful in the construction in the first instance, they would nevertheless be highly improper to depend upon as the principal support of the bridge. Under every circumstance, I am of opinion that the tubes should be made sufficiently strong to sustain not only their own weight, but in addition to that load, 2000 tons equally distributed over the surface of the platform, a load ten times greater than they will ever be called upon to support.

In fact, it should be a huge sheet-iron hollow girder, of sufficient strength and stiffness to sustain those weights; and, provided the parts are well-proportioned, and the plates properly riveted, you may strip off the chains, and leave it as a useful monument of the enterprise and energy of the age in which it was constructed.

In the pursuit of the experiments on the rectangular as well as other descriptions of tubes, I have been most ably assisted by my excellent friend Mr. Hodgkinson; his scientific and mathematical attainments render him well-qualified for such researches; and I feel myself indebted to him for the kind advice and valuable assistance which he has rendered in these and other investigations. I am also deeply indebted to yourself and the Directors for the confidence you have placed in my efforts, and for the encouragement I have uniformly received during the progressive developments of this inquiry.

But, in fact, the subject is of such importance, and the responsibilities attached to it are so great, as to demand every effort to demonstrate, calculate, and advise what in this case is best to be done. Both of us have therefore laboured incessantly at the task, and I am indebted to my friend for the reduction of the experiments which I would not attempt to weaken by a single observation.

WM. FAIRBAIRN.

MR. HODGKINSON'S REPORT.

SUMMARY OF RESULTS

Offered, in conjunction with one by William Fairbairn, Esq., M.I.C.E. to Robert Stephenson, Esq., M.I.C.E., &c. &c., for the Directors of the Chester and Holyhead Railway, on the subject of a proposed Bridge across the Menai, near to Bangor. By EATON HODGKINSON, *F.R.S.*

Having in the month of August last year been requested to render assistance, principally in a scientific point of view, with respect to the experiments to ascertain the practicability of erecting a Tubular Bridge across the Menai Straits, of sufficient strength for railway trains to pass through it with safety, I attended twice in London for that purpose: and as the experiments made there were on tubes of various forms of section, including several elliptical and circular ones, I investigated formulæ for reducing the strength of the leading ones. It appeared evident to me, however, that any conclusions deduced from received principles, with respect to the strength of thin tubes, could only be approximations; for these tubes usually give way by the top or compressed side becoming wrinkled, and unable to offer resistance, long before the parts subjected to tension are strained to the utmost they would bear. To ascertain how far this defect, which had not been contemplated in the theory, would affect the truth of computations on the strength of the tubes proposed to be used in the bridge,—and also to show whether the principles generally received could be applied with certainty in reasoning as to the strength of the bridge from that of models comparatively very small,—for these two purposes I urged the necessity of a number of fundamental experiments, which, besides supplying the wants above-mentioned, might enable me to obtain additional information to that from Mr. Fairbairn's experiments, with respect to the proportions that the different parts of the section of such a bridge ought to have, as well as what form it should be of, in order to bear the most.

Feeling that there might be objections against allowing me to follow the course I proposed, however necessary it might appear to myself, I suggested a much more limited series of experiments than now appear to me to be necessary; and, as the time consumed in getting the plates rolled and the tubes prepared, caused the experiments to be delayed till the beginning of the year, the time given me has been too limited to obtain all the facts which the few experiments proposed would have afforded.

I will now give the results, so far as they have been obtained and seem worthy of reliance, subject to correction from future experiments; beginning with the reduction of Mr. Fairbairn's experiments on the strength of tubes of wrought iron made of plates riveted together.

Cylindrical Tubes.

The strength of a cylindrical tube, supported at the ends, and loaded in the middle, is expressed by the formula

$$w = \frac{\pi f}{al}(a^4 - a_{\prime}^4),$$

where l is the distance between the supports; a, $a_{,}$ the external and internal radii; w the breaking weight; f the strain upon a unity of section, as a square inch, at the top and bottom of the tube, in consequence of the weight w; $\pi = 3{\cdot}14159$.

From this formula we obtain

$$f = \frac{wla}{\pi(a^4 - a_{,}^4)}.$$

As it will be convenient to know the strain f per square inch which the metal at the top and bottom of the tube is bearing when rupture takes place, this value will be obtained from each of Mr. Fairbairn's experiments: the value w being made to include, besides the weight laid on at the time of fracture, the pressure from the weight of the tube between the supports, this last being equal to half that weight. Computing the results, we have from

$$\left.\begin{array}{lll} \text{Experiment} & 1. & f=33456 \\ ,, & 2, & f=33426 \\ ,, & 3, & f=35462 \\ ,, & 4, & f=32415 \\ ,, & 5, & f=30078 \\ ,, & 6, & f=33869 \\ ,, & 7, & f=22528 \\ ,, & 8, & f=22655 \\ ,, & 9, & f=25095 \end{array}\right\}\text{Mean } 29887 \text{ lbs.} = 13{\cdot}34 \text{ tons.}$$

Fracture in all cases took place either by the tube failing at the top, or tearing across at the rivet-holes; this happened on the average, as appears from above, when the metal was strained 13⅓ tons per square inch, or little more than half its full tensile strength.

Elliptical Tubes.

The value of f in an elliptical tube broken as before (the transverse axis being vertical), is expressed by the formula

$$f = \frac{wla}{\pi(ba^3 - b_{,}a_{,}^3)},$$

where a, $a_{,}$ are the semitransverse external and internal diameters; b, $b_{,}$ the semiconjugate external and internal diameters; and the rest as before, w including in all cases the pressure from the weight of the beam.

Computing the results from Mr. Fairbairn's experiments, we have from

$$\left.\begin{array}{lll} \text{Experiment } 20, & f = 36938 & \text{lbs.} \\ \quad " \quad 21, & f = 29144 & \\ \quad " \quad 24, & f = 45185 & \end{array}\right\} \text{Mean } 37089 \text{ lbs.} = 16{\cdot}55 \text{ tons.}$$

Rectangular Tubes.

If in a rectangular tube, employed as a beam, the thickness of the top and bottom be equal, and the sides are of any thickness at pleasure, then we have

$$f = \frac{3wld}{2(bd^3 - b_{\prime}d_{\prime}^{\,3})},$$

in which d, $d_{\prime}$ are the external and internal depths respectively; b, $b_{\prime}$ the external and internal breadths; and the rest as before.

Mr. Fairbairn's experiment No. 14 gives by reduction

$$f = 18495 \text{ lbs.} = 8{\cdot}2566 \text{ tons.}$$

This is, however, much below the value which some of my own experiments give, as will be seen further on.

The value of f, which represents the strain upon the top or bottom of the tube when it gives way, is the quantity per square inch which the material will bear either before it becomes crushed at the top side or torn asunder at the bottom. But it has been mentioned before, that thin sheets of iron take a corrugated form with a much less pressure than would be required to tear them asunder; and therefore the value of f, as obtained from the preceding experiments, is generally the resistance of the material to crushing, and would have been so in every instance if the plates on the bottom side (subjected to tension) had not been rendered weaker by riveting.

The experiments made by myself were directed principally to two objects:—

I. To ascertain how far this value of f would be affected by changing the thickness of the metal, the other dimensions of the tube being the same.

II. To obtain the strength of tubes, precisely similar to other tubes fixed on,—but proportionately less than the former in all their dimensions, as length, breadth, depth and thickness,—in order to enable us to reason as to strength from one size to another, with more certainty than hitherto, as mentioned before. Another object not far pursued, was to seek for the proper proportion of metal in the top and bottom of the tube. Much more is required in this direction.

In the three series of experiments made, the tubes were *rectangular*, and the dimensions and other values are given below.

Length of tube.	Weight of tons.	Distance between supports.	Depth of tube.	Breadth of tube.	Thickness of plates of tube.	Last observed deflection.	Corresponding weight.	Breaking weight.	Value of *f* for crushing strain.
ft. in.	cwt. qrs.	ft. in.	inches.	inches.	inch.	inches.	tons.	tons.	tons.
31 6	44 3	30 0	24 nearly	16 nearly	·525	3·03	56·3	57·5	19·17
31 6	24 1	30 0	24 ,,	16 ,,	·272	1·53	20·3	22·75	14·47
31 6	10 1	30 0	24 ,,	16 ,,	·124	1·20	5·04	5·53	7·74
	lbs. oz.						lbs.	lbs.	
8 2	78 13	7 6	6 ,,	4 ,,	·132	·66	9,416	9,976	23·17
8 2	38 11	7 6	6 ,,	4 ,,	·065	·32	2,696	3,156	15·31
8 2	...	7 6	6 ,,	4 ,,					
4 2⅛	10 12	3 9	3 ,,	2 ,,	·061	·435	2,464	2,464	24·56
4 3¼	4 15	3 9	3 ,,	2 ,,	·03	·13	560	672	13·42

The tube placed first in each series is intended to be proportional in every leading dimension, as distance between supports, breadth, depth, and thickness of metal, and any variations are allowed for in the computation. Thus the three first tubes of each series are intended to be similar; and in the same manner of the other tubes, &c.

Looking at the breaking weights of the tubes varying only in thickness, we find a great falling off in the strength of the thinner ones; and the values of *f* show that in these—the thickness of the plates being 0·525, 0·272, 0·124 inch—the resistance, per square inch, will be 19·17, 14·47, and 7·74 tons respectively. The breaking weights here employed do not include the pressure from the weight of the beam.

The value of *f* is usually constant in questions on the strength of bodies of the same nature, and represents the tensile strength of the material, but it appears from these experiments that it is variable in tubes, and represents their power to resist crippling. It depends upon the thickness of the matter in the tubes, when the depth or diameter is the same; or upon the thickness divided by the depth when that varies. The determination of the value of *f*, which can only be obtained by experiment, forms the chief obstacle to obtaining a formula for the strength of tubes of every form. When *f* is known the rest appears to depend upon received principles, and the computation of the strength may be made as in the 'Application de la Mécanique' of Navier, part 1, Article IV.; or as in papers of my own in the Memoirs of the Literary and Philosophical Society of Manchester, vols. iv. and v., second series. I have, however, made for the present purpose further investigations on this subject, but defer giving them till additional information is obtained on the different points alluded to in this report; and this may account for other omissions.

In the last table of experiments the tubes were devised to lessen or to avoid the anomalies which riveting introduces, in order to render the properties sought for more obvious. Hence the results are somewhat higher than those which would be obtained by riveting as generally applied.

The tube, 31 feet 6 inches long, 24 cwt. 1 qr. weight, and ·272 inch in thickness of plates, was broken by crushing at the top with 22·75 tons. This tube was afterwards rendered straight, and had its weak top replaced by one of a given thickness, which I had obtained from computation; and the result was, that by a small addition of metal, applied in its proper proportion to the weakest part, the tube was increased in strength from 22·75 tons to 32·53 tons; and the top and the bottom gave way together.

If it be determined to erect a bridge of tubes, I would beg to recommend that suspension chains be employed as an auxiliary, otherwise great thickness of metal would be required to produce adequate stiffness and strength.

EATON HODGKINSON.

CORRESPONDENCE, &c. (CONTINUED).

AFTER the completion of the experiments on the tube with a corrugated top, and the development of the principle upon which the large ones should be constructed, came the question of security, in reference to the tubes supporting themselves, independently of auxiliaries in the shape of chains. It will be observed from the report that I was anxious to clear the tube of the incumbrance of chains, which, it must be borne in mind were intended, from the first, not only for the support of the tubes, but for the purpose of carrying them forward from the platforms, on which they were to be built, across their respective spans to their final positions on the piers*. A difference of opinion existed on this subject, but Mr. Stephenson had made up his mind, and hence the suggestions contained in the following letter, dated from Belfast, February 23rd, 1846.

MY DEAR SIR, Belfast, Feb. 23rd, 1846.

I have been considering the principle upon which you purpose attaching the chain, for the support of the tube; and with every deference to your judgement, I am almost inclined to differ with you on that point.

It appears to me that the great and important consideration is, to relieve the strain upon the tube. It is quite clear that a series of chains on each side of the plates, well-fitted and tightly screwed up, would tend to stiffen the sides, and give greater rigidity to those parts. This is however not what is wanted; *the rigidity is required on the top side*; as in all the experiments the sides seldom get out of form, unless distorted by the crushing of the top side. Under these circumstances, the stiffening in my opinion should be on the top platform of the tube: and the only remedy yet found for these defects, is the corrugated or tubular distribution of the material in that part.

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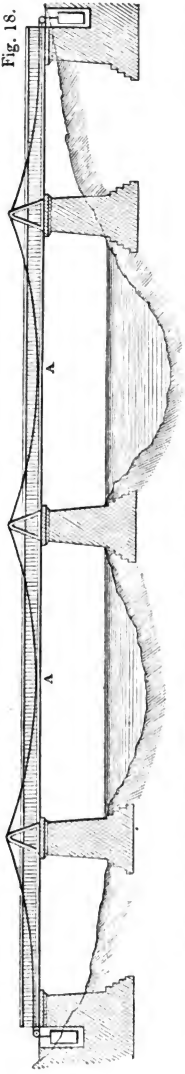
* Mr. Stephenson had a most ingenious method of accomplishing this object. See observations prefixed to my letter, dated July 15th, 1846.

The great superiority of this form of tube is clearly demonstrated by experiment 29 in the table, where the strength was increased nearly threefold, from the rigidity introduced by this form of top, which rendered the resistance to compression, as nearly as possible, equal to that of ultimate tension below. I have no doubt, when Mr. Hodgkinson has once attained a satisfactory formula for the reduction of this form of tube, that the result will, probably, not be far from the maximum of the section of greatest strength.

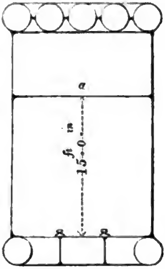
I should conceive that, in making the tube with a corrugated top, the chains should be attached in the following form (fig. 18), and of sufficient strength to sustain a load of from 1500 to 2000 tons. This would be sufficient to sustain the platform for erecting the tubes, and by suspending weights at each end, probably equal to 300 tons; the tendency would then be to support the middle of the tubes at AA, and to enable us to lighten them, as much as might be consistent with their perfect safety, even without the chains. For these objects I would respectfully suggest, that the chains should not act upon the tubes at any point but the centres AA; and that, for the exclusive purpose of relieving the tubes of a part of their own weight. With respect to the tube getting out of shape, I have no fear of such an occurrence, provided the corrugated top be sufficiently rigid; and, in order to stiffen it still more, it may be advisable to introduce a hori-

Fig. 18.



zontal screen of iron, at a height of 15 feet above the rails, uniting the two sides, and forming a gallery as shown in the sketch at *a* (fig. 19).

Fig. 19.



These are however matters of detail that can be entered into hereafter. For the present, I shall be glad if you will reconsider these points, and let me have your opinion upon them. Mr. Hodgkinson is very desirous of prosecuting the experiments still further, for the purpose of attaining, not only a correct formula for reducing the tubes with the corrugated tops, but to ascertain whether a still better form may or may not be obtained. As soon as these experiments are completed, I shall have a tube made exactly $\frac{1}{6}$th the dimensions of the bridge; when I have no doubt you will find the views, I have ventured to express, fully borne out by experimental results. For these objects I could wish you to be present at the tests, in order more clearly to determine, and with still greater accuracy, the proportions, as well as the amount of auxiliary support it will require.

Yours faithfully,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

From the frequent meetings and conversations which took place from time to time, it was evident that Mr. Stephenson's mind was uneasy, and that he even entertained doubts and fears as to the security of the bridge. This is scarcely to be wondered at, as up to that time his avocations were such as precluded the possibility of his being present to witness a single experiment on the strengths of the tubes. Had Mr. Stephenson seen those experiments which produced the best results, it might have given a different turn to his thoughts, and inspired much greater confidence than he could possibly have derived from written reports.

Mr. Stephenson was present at one or two of the experiments afterwards made on the model tube, and, after witnessing them, his fears were in a great degree removed; he then determined to abandon the use of auxiliary chains, and from that time, October 1846, to the completion of the Conway Bridge, he relied with confidence on the strength of the tube itself, and attached a proper degree of importance to the results of my earlier experiments.

Mr. Stephenson writes from Londonderry, as follows:—

MY DEAR SIR, Londonderry, Feb. 23rd, 1846.

It was my intention on my return to London to have called and spent a day with you in Manchester, but this is now uncertain. I therefore venture to trouble you with a brief note, to say that I hope Mr. H., in devising his future experiments, will keep in view the importance of introducing some tubes with corrugated tops, for it is clear that we must look to this description of tube for practical purposes. After what passed at our last interview at Manchester, I hope Mr. H. will see little difficulty in bringing such forms within the scope of his formula. Now one word about time, which will very shortly press me into a corner. I have engaged to have the plan and specification of the masonry ready in a fortnight, and although nothing specific need be determined in that time, as I can meet this by stipulations, yet, in a month the masonry must be actually commenced, and previous to that, it is exceedingly desirable that something decided should be ascertained as to the dimensions of the tubes. If you could accelerate Mr. H's proceedings, so as to effect this, I should feel much obliged.

Reflection upon the arrangements of the tube and chains, has, in my mind, cleared up several little difficulties, and I now feel quite comfortable about the whole affair. We must begin to think of arranging a complete boiler-yard at the Straits. We shall require, I think, four of your patent riveting machines. Will this be sufficient for riveting 2000 tons of plates in twelve or eighteen months? On my reaching London I will write again, when I hope to be able to arrange to meet you in Manchester.

Yours sincerely,

William Fairbairn, Esq. ROBERT STEPHENSON.

Here it is obvious, that there is a desire, on the part of Mr. Stephenson, to relax in his former determination respecting the application of chains; but as the point was far from being definitely settled, my next letter treats of the best method of attaching them.

My dear Sir, Manchester, February 28th, 1846.

Under the impression that you were in London, I wrote you a somewhat hasty letter from Belfast on Monday last, which however was posted in Glasgow on the following day. In that communication I gave you freely, but roughly, my impressions regarding the use and application of the chains to the proposed bridge. I would now correct that part of my letter which applies to the attachment of the chains to the lower side of the tube, when I stated that they should not be in connexion with the tubes at a greater distance than 20 or 25 feet from each side of the centres. Now, on more mature reflection, I think we might with safety adopt your plan of attaching the links on both sides for at least 50 feet each way, and fix or screw the chains rigidly to the sides and bottom for this distance. Beyond this I would have no vertical connexion in the way of support, but depend entirely upon the tension which the chains would uniformly receive by the suspended weights always acting upon them.

Again, I should have mentioned that the sector, or fulcrum, over which the tension-weights revolved, should be fixed to the tube or its frame, in order not only to assist in supporting the middle of the bridge, but to bring the top part as much as possible into a state of tension, on the same principle as the tail-piece to your cast-iron bridge, which operates more or less in that direction.

All these are important points and require mature consideration, and I should be most happy to see you either here or elsewhere, in order that we might lay our heads together and determine what is best to be done. As soon as these matters are satisfactorily decided, the whole of the masonry may be proceeded with, and so far as I can see at present, I am persuaded that the tubular or corrugated top with a well-proportioned bottom must be the shape of the bridge.

I have not seen Mr. Hodgkinson since my return, but I hope to see him tomorrow, and to determine what further experiments can be made. What is chiefly wanted is an accurate formula for the reduction of the experiments already made, and probably a few more, to see if Mr. Hodgkinson can throw any further light upon the subject as to form and distribution of the material. After these are accomplished I will make the model-tube, as already proposed; and I have no doubt, but a carefully conducted experiment of this kind (upon a much larger scale) will exhibit some further developments, as respects the puckering principle, which at present appears to be the only difficulty.

I have said nothing on the subject of construction, but presuming it is your wish that I should take charge of the whole of the iron-work, and relieve you as much as possible of the detail, I should be glad to have your advice, and to act under it to the utmost of my power. In a work of this kind I should have to devote a considerable portion of my time to the undertaking, and having to abstract that time from other avocations, I could not do so but upon a salary, such as you would recommend as a fair remuneration, or a per-centage upon the weight or cost of the iron-work. These are however matters on which I should be guided by your advice. In the meantime, I am prepared, whenever you are at leisure, to go into the subject of the work-sheds, in order to be prepared and ready for action when you give orders to that effect.

I am, my dear Sir, faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

Fears were entertained by Mr. Hodgkinson and others, that the sides of the tube would collapse for want of stiffness. In my letter of February 23rd, 1846, I proposed to meet the difficulty, by connecting the sides of the tube by a horizontal sheet-iron tie-plate or framing, at a height of about 15 feet above the rails. Representations of the danger and insecurity, arising from this imaginary point of weakness, were urged to such an extent, that had they been acted upon, they would have occasioned serious pecuniary loss to the Chester and Holyhead Railway Company, and might have delayed, for some time, the introduction of a form of bridge, which has already proved itself of peculiar value, in enabling the railway engineer to carry his line over streams and ravines, which at one time appeared impassable.

The time had now arrived when it became absolutely necessary that we should proceed with the construction of the tubes. Six months had nearly elapsed, waiting for the results of Mr. Hodgkinson's investigations; but it appears that his labours were advancing, under circumstances unsatisfactory to himself, and embarrassing to others. Mr. Stephenson was urgent, and the Directors were impatient in consequence of the delay: under these circumstances, the model tube, which had stood over so long, had eventually to be made, and the experiments (which have resulted in the existing construction) proceeded with, independently of any aid from Mr. Hodgkinson.

In this stage of the proceedings, connected with the undertaking, it would be improper to withhold the following letter, addressed to Mr. Stephenson. It appears from this letter, that Mr. Hodgkinson felt himself aggrieved in not being "called in at the commencement, but after the apparatus was formed and about one-third of the experiments made; and then not to direct, but to advise and compute." Without giving up my proper claim to the important facts, which my experiments had established,—important not only in relation to the construction of the Menai Bridge, but valuable as matters of scientific research,—I made concessions, without number, to gain Mr. Hodgkinson's cooperation, but without effect, and there being then no other resource, I at length resisted his demands, and proceeded with the experiments in my own way. The assertion, that "many of these experiments were made from [his] own suggestions," is not the fact; no doubt Mr. Hodgkinson persuaded himself into the belief that he had done so*. The truth is, from the very first he cautiously withheld his views, and that for reasons given in his own letter: he observes, "Should it be asked, why I did not make these objections at an earlier date; I would state, that, although consulted, I was not called in," &c.

This letter, although written in a hostile spirit, doubtless contains the key to a number of valuable experiments, subsequently conducted by Mr. Hodgkinson, the results of which were given to Mr. Stephenson, but were never communicated to me. In fact, they had no further connection with the construction of the existing bridges, than the influence they may have had on Mr. Stephenson's mind, before he approved of the plans and suggestions which I had made.

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* This statement, on the part of Mr. Hodgkinson, refers to suggestions, which he imagines to have made, when he was at Millwall on the 18th of September 1845, witnessing the experiments then in progress on tubes having cells on the top. See the letter to Mr. Stephenson, dated September 20th, 1845, and those from Mr. Ross and Mr. Graham in the Appendix.

In recording these facts, there is no intention to depreciate the value of Mr. Hodgkinson's researches; they are, I have no doubt, highly valuable, as every experiment must be, which emanates from so able a mathematician.

It must be evident, however, that none of Mr. Hodgkinson's experiments (excepting only those recorded in the Report*) could have had any effect upon the construction of the tubes for crossing the Conway and Menai Straits. The form and proportions of these tubes were partly determined from the experiments made at Millwall before Mr. Hodgkinson was called in, and finally from those made on the Model Tube (without Mr. Hodgkinson's approbation), which decided the question without reference to the experiments of any other person whatever.

14 Crescent, Salford, Manchester,
March 10th, 1846.

My dear Sir,

As I have had no direct correspondence with you, except when you were at Manchester, I should perhaps have acquiesced in my friend Mr. Fairbairn continuing to be the medium of communication, had I conceived that he could in all cases have been so with propriety.

The application to me to take a part in devising experiments with respect to the bridge having come from him, with your sanction, I thought that he was the proper person to correspond with you.

Mr. Fairbairn has however now completed, with my suggestions, a considerable number of experiments, and on the evidence from these, and a few made by myself, you have made a report to the Directors of the Chester and Holyhead Railway.

You conclude in your report, that the form of the tube, and the distribution of the material, have been finally determined, and that the only important question now remaining to be solved, is the absolute ultimate strength of the tube of any given dimensions, this being approximately solved by the experiments already completed.

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* See the Reports to the Directors of the Chester and Holyhead Railway, p. 42.

Finding that you had already drawn these conclusions, I felt some alarm lest they should not be borne out by the facts when fully explained.

I will now state what appears to me to be wanted, and give the reason for my doubts.

The experiments made by Mr. Fairbairn on circular, elliptical and rectangular tubes, give considerable information. Those on the rectangular section are numerous, and I hope good; but do not, I think, show, except approximately, the proper distribution of the metal, as to the strength of the top, bottom and sides for the bridge.

As many of these experiments were made from my own suggestions, following out the reasonings employed in my experiments to ascertain the best form of iron beams (Memoirs of the Literary and Philosophical Society of Manchester, vol. v.), I am the more entitled to make these objections.

The results of the experiments hitherto made, do not perhaps show indisputably that the rectangular form is the best, though I believe it to be so from other considerations.

The proper strength of the metal in the sides is an important element for giving adequate stiffness to the tube. If this be not attended to, the tube will give way by bending *laterally*, a tendency very commonly observed in experiments on wrought iron.

The apparatus used for the experiments was very ingeniously constructed, and well-adapted to avoid shaking in loading and unloading the tube; but the tensile force being, from the nature of the apparatus, always exerted in one vertical line, the tendency of the weight was to draw the tube in that direction only, and to prevent it getting out of the vertical line, which is so frequent a cause of failure in wrought iron girders, that it is almost impossible to keep them in shape when heavily loaded.

It is probable that some of the tubes tried by Mr. Fairbairn would have been broken with less weights than they did bear, if they could have yielded in the weakest direction.

In the future experiments it will be desirable to apply the pressure so as to allow the tube to yield in the direction of greatest weakness.

In the construction of a tube to bear the greatest weight for the quantity of material, the thickness of the sides being found sufficient to preserve its form, and the top and bottom being of sufficient strength for the purpose required, and adapted to each other so that both should be ready to give way at the same time, the top being considered as a solid mass; this top should then be replaced with circular tubes of wrought iron of equal strength with it to resist crushing.

To enable this to be done, I would suggest the propriety of making a few experiments upon the power of wrought iron to resist a crushing force; it would effect the substitution above, and supply important information in addition to that in my memoir (Philosophical Transactions, 1840).

The tube must not be uniform, but varying in thickness and strength, from the ends towards the middle in the proper proportion.

To extend further the experiments on the strength of similar tubes, of which an abstract is given in my report, I would beg to suggest that three other tubes, half as large again as the largest there employed, be constructed; say 45 feet between the supports, 3 feet deep and 2 feet wide nearly, the thicknesses varying as before.

To make these and other experiments, which I conceive will be necessary, a suitable apparatus for breaking large beams, as a large cast-iron lever to bear 150 tons, would have to be constructed with as little expense as possible, to give adequate security; the experiments to be made so as to allow the tube to yield in the direction of greatest weakness.

The tubes tried in London required only about ten or eleven tons each at the utmost to break them, and more than half were broken with under five tons. They furnish valuable introductory information, but are much too small for the purpose as ultimate experiments. They would require to be repeated on a large scale, obviating the objections pointed out above, before they could with propriety be applied to the bridge. Should it be asked, why I did not make these objections at an earlier date, I would state that, although consulted, I was not called in at the commencement, but after the apparatus was formed and about one-third of the experiments made, and then not to direct, but to advise and compute.

A consideration of the importance of this inquiry, and of the serious consequences of a failure, has impelled me to make this candid statement.

I am, my dear Sir, yours very truly,

Robert Stephenson, Esq. EATON HODGKINSON.

Mr. Stephenson at once acceded to Mr. Hodgkinson's request, viz. that he might have his own way in conducting his experiments. From this time to the completion of the first Conway tube, there was no communication of the slightest importance made to me relative to Mr. Hodgkinson's proceedings.

My dear Sir, London, March 14th, 1846.

I am ashamed at the apparent neglect which my silence must have given rise to, but you are tolerably well aware of my avocations, and will no doubt put a liberal construction upon it. Mr. Hodgkinson is getting nervous, and has forwarded me some strictures upon what has already been done. I have this day written to him, giving him an unqualified assent to his undertaking any reasonable series of experiments that he may deem advisable for clearing his mind.

My own mind is made up, but I would by no means limit the inquiry; on the contrary, I am most anxious that Mr. Hodgkinson, as he has become connected with the matter, should proceed in any way that may render the subject of the transverse strength of tubes clear, and as unequivocal as his investigation of beams. He appears to wish to have them under his own control. I am sure you have no objections to this: I have none whatever. On this part of the subject, as well as those you start in your two last letters, I must see you personally. With regard to your remuneration, rely upon it, it is my wish to make this perfectly satisfactory to you. It has been named to the Board, and any arrangement you and I may make will be no doubt sanctioned. I am engaged considering all your suggestions, and when some sketches are completed which are now in hand, I shall make a point of seeing you. If possible, I shall endeavour to spend a day with you in Manchester next week.

Yours sincerely,

William Fairbairn, Esq., Manchester. ROBERT STEPHENSON.

The whole of the following letters up to March 21st, principally refer to the unfortunate misunderstanding which took place with Mr. Hodgkinson. This portion of the correspondence must, therefore, be understood as having transpired under circumstances of considerable excitement.

My dear Sir, Manchester, March 14th, 1846.

Mr. Hodgkinson has shown to me a letter addressed to you, on the subject of the experiments which I had the honour of making, relative to the proposed bridge over the Menai Straits. It is true that Mr. Hodgkinson has obtained a decidedly high standing in the mathematical and scientific world, but he does not stand pre-eminent in every department; and so far as regards the experiments made in London, they were done independently of Mr. Hodgkinson, and (in the absence of all previous knowledge) to the best of my ability. I am therefore fully prepared to abide by the results.

In conducting these experiments, I had your confidence and support, and having satisfied myself as to the strongest sectional form of tube (or a close approximation to it), I deemed it necessary, not only to pursue the investigation (which daily increases in interest) to the fullest extent, but still further to elucidate the subject in a scientific point of view. I obtained your consent to call in the assistance of Mr. Hodgkinson, than whom, I am sure, there is none more capable of giving sound and useful advice. Knowing Mr. Hodgkinson's feelings on these matters, I used every endeavour to make the subject agreeable to him, and we have already gone so far as to give him a prominent position in what has been done; I find, however, that he is not satisfied, and rather than injure a question of such importance in a petty contest about priority, I would prefer withdrawing from the investigation altogether. Mr. Hodgkinson is willing from kindly feelings to render any assistance, but he seems desirous and fully determined to have it all his own way, and in his own name. Under these circumstances I shall wait your further instructions, and remain, my dear Sir,

Yours faithfully,

Robert Stephenson, Esq. William Fairbairn.

P.S. I may mention that Mr. Hodgkinson looks upon the present investigation as an abstract scientific inquiry, having, as a matter of course, reference to the bridge. Such an investigation, however, may spread far, and I am not sure (without your consent) how far I am justified in putting the Company to such a large expenditure of time and expense.

My dear Sir, [No date, about the 16th of March 1846.]

I have written to Mr. Hodgkinson, and hope he will continue his experiments. I shall deeply regret their being discontinued after such an expenditure as has now been incurred; and in addition to this, I fully anticipate that he will extract from them much that is both interesting in a scientific point of view, and valuable to us as we progress with the Britannia bridge. I really deplore the jealousy that this investigation has given rise to. On the former occasion, when symptoms of this kind presented themselves, I hoped that the arrangement which was made would have worked satisfactorily. The conducting of a few practical experiments by you and me, surely ought not to excite such feelings in Mr. Hodgkinson's mind, as now seems to be the case. I have told Mr. Hodgkinson that the Directors consider me as their engineer, responsible for the works of the railway being ready for the public by the time specified in their reports to the shareholders, and I have no choice; some risk must be incurred, or the pecuniary loss to the Company must be very serious.

I enclose you a copy of my note to Mr. Hodgkinson.

Yours sincerely,

William Fairbairn, Esq., Engineer, Robert Stephenson.
Manchester.

My dear Sir, London, March 17th, 1846.

I do not know that you have seen a pamphlet containing our joint, or rather separate reports, but I send you one, being the first I have seen. Surely this shape gives us all our proper share of merit. I wrote to Mr. Hodgkinson yesterday, and explained to him the absolute necessity there was for despatch in this matter, saying at the same time that if he felt any indisposition to pursue the matter further, I should be glad to know it at once, in order that other arrangements might forthwith be made.

The plans and specification for the masonry are laid before the contractors today; the letting will take place next Wednesday. This will, I trust, be sufficient to satisfy Mr. Hodgkinson that despatch is necessary, and I trust that your other avocations will permit you to accelerate the construction of tubes such as may meet Mr. Hodgkinson's views.

If he be reluctant to enter upon the investigation, it is of importance that I should know it at once.

Yours faithfully,

William Fairbairn, Esq. Robert Stephenson.

P.S. Allow me, in closing my letter, to say how much pleased I was with your liberal treatment of the *little difference* which has taken place between us and Mr. Hodgkinson. Neither of us, I am sure, want to deprive him of the merit of any abstract mathematical investigation which the experiments may lead us to; but, on the other hand, he cannot suppose that such an investigation is the all-important consideration which the question involves. R. S.

My dear Sir, Manchester, March 18th, 1846.

I am truly sorry that any misunderstanding should have taken place on a subject of such importance as that now under consideration. I am far from assuming to myself an undue importance in an investigation for which you were so well-qualified. I must, however, claim for myself some knowledge upon the subject, at least practically; and finding myself under considerable responsibility as to the ultimate success of the undertaking, I cannot in justice surrender the position to which I consider myself justly entitled. I can assure you I am far from wishing to detract, even the smallest fraction, from your deservedly well-earned reputation. It is my interest and my earnest wish to add to its renown; and whatever may be the issue of the present misunderstanding, you will always find in me your steadfast friend.

In order to satisfy all parties, and prevent future uneasiness, it is proposed by Mr. Stephenson, and agreed to by myself, that you shall make whatever experiments you deem necessary (and I will give every facility for the purpose), on condition that whatever is done by you, as well as myself, shall merge in a joint report, to which the names of all the three shall be attached. If you agree to this, I have instructions to proceed with the least possible delay, in order to enable the works to be proceeded with, and that upon principles satisfactory to all parties.

I am leaving for London again this afternoon, and will be glad if you will favour me with a note per post, addressed Fenton's Hotel, St. James's Street, Westminster, where I shall be till Friday. Hoping these arrangements may be satisfactory,

I am, my dear Sir, always affectionately yours,

Eaton Hodgkinson, Esq. William Fairbairn.

My dear Sir, Salford, March 21st, 1846.

I send you a copy of Mr. Stephenson's letter, according to which I have engaged to act; for his renewed offer of last Wednesday was agreeable to it, and more explicit.

(*Copy.*)

"24 Great George Street, Westminster,
"My dear Sir, March 14th, 1846.

"I feel much obliged by your candid exposition of your views in reference to the bridge experiments, and should have replied to your kind note earlier, had not urgent engagements prevented me.

"I have now to ask the favour of your proceeding with such a series of experiments as you may think advisable, to clear up any doubts you may have. I wish the thing to be thoroughly sifted by a scientific man like yourself, although my own practical conclusions, from a careful study of iron ship-building, leave my mind free from apprehension. In a matter however of such importance, this is scarcely sufficient. I trust, therefore, you will consider the scientific and experimental investigation under your own control, as I am far from wishing you to act as second either to Mr. Fairbairn or myself in this department of the inquiry: I shall be glad to hear that the arrangement is satisfactory. In the course of next week I hope to be able to spend a day with you and Mr. Fairbairn in Manchester.

"I am, my dear Sir, yours faithfully,

"Robert Stephenson."

You must look at my experimental efforts not as in opposition to your own, but as a friendly inquiry to corroborate and extend them so far as they may run parallel to each other, and to bring the matter more within the reach of the mathematician. In this inquiry Mr. Stephenson and yourself will be associated with me, though the authorship and the direction of the experiments will be my own. They will be published in my name, as your own experiments will be in yours. The question to be solved by experiment is a large one, and you have taken one department, I shall take another; but when I have done my best, the subject is of such magnitude, that we shall be so far from having exhausted it, that Mr. Stephenson would be justified in calling in some other persons, if he can obtain them, to pursue it further.

I am, my dear Sir, yours affectionately,

William Fairbairn, Esq. Eaton Hodgkinson.

At the close of this correspondence Mr. Hodgkinson was left to pursue his experiments in his own name, and in the form most agreeable to himself. Some valuable facts were obtained by him relative to the powers of wrought iron to resist compression; but these results, being limited in their application, formed no part of the calculations referred to in the following letters. Besides, a number of Mr. Hodgkinson's most important experiments were not then made, and after they were completed, the results were either sent to London or were retained by him for his own immediate use. Having failed in obtaining Mr. Hodgkinson's assistance, I proceeded, in the following letter, to determine, from my own experiments, the relative proportions of the sectional areas of the top and bottom sides of the large tubes. The calculations are based upon experiments 15, 16, and 29, which gave the best results. This last-mentioned experiment was made upon the tube which contained the longitudinal cells, and which was described in my letter to Mr. Stephenson (page 19).

MY DEAR SIR, Manchester, April 3rd, 1846.

You would receive a copy of the experiments, as taken from my notebook; they are in a rough state, but may for the present be useful, or till such time as they are reduced and brought into a more tangible form.

As some months may elapse before anything definite is obtained from Mr. Hodgkinson, I have deemed it advisable to deduce from my own experiments, results which I trust will be satisfactory, at least so far as respects the section of greatest strength, and the proper distribution of the material in the different parts of the bridge. It has already been determined by experiment, that the strongest section yet obtained is that of the rectangular form; it has also been ascertained that the upper and lower sides of the tube bear certain relative proportions to each other, and that in such ratio as will correctly balance the resisting forces of compression on one side, and of extension on the other. On this part of the subject, it is a consideration of much importance to have (in a structure such as the Menai Bridge) neither more nor less material than is absolutely necessary to retain the parts in equilibrium by adding to its strength; and that under every circumstance, and every description of strain to which it may be subjected. This appears to me to be the ultimatum of every experiment; and although we may not, in the present stage of the inquiry, have attained the same exactitude which future experiments may develope, we have nevertheless acquired certain fixed laws which determine the form and clearly establish the proportions of the relative strength or distribution of the material in the different parts of the structure. These may be determined from the experiments as follows.

On consulting Experiments 15, 16, and 29, on the rectangular tubes, it will be found that they approximate to each other in a certain ratio, as to the thickness of their top and bottom sides. The first, it will be observed, broke by tearing asunder through the rivet-holes; the second was crippled by "buckling" on the top; and the last was fractured by the sides tearing from the top and bottom at one and the same time. Now, if we take the mean thickness of the plates used in these experiments, I apprehend we shall have a fair proportional of the quantity of metal which should be used respectively in the top and bottom sides of the large tube, in order to obtain the section of greatest strength. The mean of the top and bottom sides of the tubes experimented upon was,—

Experiments.	Area of top side in inches.	Area of bottom side in inches.
15.	0·142	0·075
16.	0·269	0·149
29.	0·230	0·180
Mean . .	0·213	0·135

being in the ratio of 213 to 135, or as 10 to 6 nearly*. From this it appears that the top and bottom sides of the bridge should approach in the areas of their respective sections, as nearly as possible, to the above

---

* The same result may be obtained in the following manner:—

Experiment 15, ratio of the top and bottom areas	1·893 : 1
,, 16, ,, ,, ,, ,,	1·805 : 1
,, 29, ,, ,, ,, ,,	1·278 : 1
Mean ratio.........	1·659 : 1
or	10 : 6

proportions; and although the corrugated or tubular top may give greater strength, it would however be prudent, in the distribution of the material, to give the top side its full share, as weakness was almost invariably present in that part, and the strength of the bottom on the large scale might be increased, when the riveting is more perfectly executed. These proportions are irrespective of the vertical sides, which in my opinion should be as light as possible, or at least so far as a sufficiently rigid connexion can be established between the two retaining sides of tension and compression.

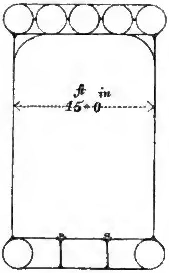
Taking the proportions given above, and assuming the tube in its full size to be 400 feet span, 26 feet deep, and 15 feet wide, we should then have, in adopting the following section for the area in inches on the top side six horizontal tubes, each 3 feet in diameter and ¾-inch thick plates,

	square inches.
$6 \times \pi.36 \times \frac{3}{4}$	$=509$
Top covering plate 15 feet broad and ½ inch thick, $180 \times \frac{1}{2}$.	$=90$
Under tube connecting plate, 15 feet $\times \frac{5}{16}$ inch thick	$=56$
Equal to .	655

of area for the side resisting compression.

Taking the tubular top at 655 square inches the bottom would then require to be as under.

Fig. 20.



Two tubes each 3 feet in diameter and ¾-inch thick plates, same as those above	=169·5	square inches.
Bottom (double) plates 15 feet wide, and 1 inch thick	=180·0	„
Two longitudinal ribs supporting rails and platform, 2 feet deep and ¾ths of an inch thick .	= 36·0	„
Top platform or roadway, 15 feet wide, and composed of plates $\frac{5}{16}$ths of an inch thick .	= 56·0	„
Total area in inches . .	441·5	„

The proportional areas of the top and bottom sides of the tube would, therefore, be (in the middle) as the numbers 655 and 441·5 respectively, or as 100 to 67 nearly.

For the present I must abandon the inquiry, but I will recur again to it in the course of a day or two, when I will endeavour further to elucidate the subject, by reference to Mr. Hodgkinson's inquiries into the strength of cast-iron beams, which bear directly upon that part of the question, and refer also to the distribution of the material in other parts of the tube.

My present object is to deduce from the experiments the strongest form of section *in the middle of the beam*, which, once attained, the other must follow as a matter of course.

I am, my dear Sir, sincerely yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

Time had now become of great importance, and finding it impossible to remain any longer inactive, it was decided to proceed with such formulæ and calculations as could be deduced from the experiments already made. In my next letter, I endeavour to complete the calculations and practical conclusions commenced in my last communication.

MY DEAR SIR, Manchester, April 6, 1846.

In my letter of the 3rd you had the characteristics deduced from experiment, as to the properties and shape, of the material forming the upper and lower sides of the tube. From that statement, it is obvious, that in order to obtain the section of greatest strength, $\frac{4}{10}$ths more material was requisite on the top, to prevent "crimping," than what was wanted on the bottom side, to resist tension. In these computations, you will please to observe, that the proportions between the resisting forces had reference to every point in the beam; but the thickness of the plates applied exclusively to the middle part, which is subject to the greatest strain. In these calculations, it must be borne in mind, that the plates, or rather the sections of double plates, must be considerably more at the centre than any other part of the tube; from that point they will diminish in certain definite proportions, as they approach the piers on each side, following the same law (as nearly as possible) as laid down in Mr. Hodgkinson's treatise on cast-iron beams. In effecting this diminution, it will be necessary to do so with caution, and here we should have to deviate from theory so far as to retain rigidity and prevent crimping, which is the chief and almost only difficulty we have had to contend with.

Judging from the experiments, I should be inclined to recommend, as proportionals of the different sections, the following, calculated from the middle of the tube each way in the direction of the piers.

Assuming the span to be 400 feet, and taking the middle sections of the top, bottom, and sides as their respective numbers 655, 441 and 150, the other sections, five in number on each side, would be as follows :—

	Area of top.	Area of bottom.	Area of sides.
	Sq. inches.	Sq. inches.	Sq. inches.
Section in the middle . . .	655	441	150
Section 40 feet on each side	614	413	150
Section 80 feet on each side	573	385	150
Section 120 feet on each side	532	357	150
Section 160 feet on each side	491	329	180
Section 200 feet (pier) . . .	491	329	240

In these calculations I have taken the tube at 26 feet in height, and the vertical sides each 20 feet deep and $\frac{5}{16}$ths of an inch thick. The sides would, however, be thickened considerably as they get near the piers, where I should recommend $\frac{1}{2}$-inch plates, in order to retain the form, and give stiffness, at the point of attachment, to the cast-iron frame imbedded on the piers. Having endeavoured, as far as the experiments extend, to reduce them to some definite form, subject to such conditions as may be necessary, as our knowledge of the subject becomes more extended, I would now direct attention to the strengths, which in these proportions I have endeavoured to establish, and which I am persuaded is not overrated, but much under what the tube is capable of bearing. I am sorry I have not the benefit of Mr. Hodgkinson's formula; but conceiving that a hollow beam follows the same law as a solid one, when subjected to a transverse strain, I have attempted to seek for the formula in a very simple way, and which, I am convinced, is not far from the truth.

If you will refer to my last experiment (29) with the corrugated top, you will find that it supported 10 tons, exclusive of its own weight, before yielding to the strain. Now, according to Mr. Hodgkinson's rule, it should, in a span of 19 feet, if made of cast-iron with a flange of 2 inches area on the bottom side, have given way with $3\frac{1}{2}$ tons. We, however, in this experiment, carry 10 tons before the buckling and tearing commences. Consequently we have an increase of strength in the difference of material alone of 10 to 3·5. Let us now assume, that the

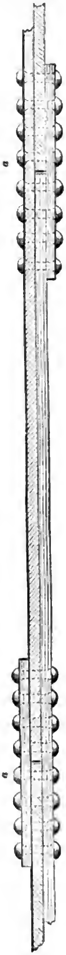


Fig. 21.

large tube is a fair proportional of experiment 29, and we then have,—

For the area of the bottom side 441·5 inches.
For the depth of tube . . . . . . 312 inches.
Distance between the supports 400 feet.

Hence $\frac{26 \times 441{\cdot}5 \times 312 \times 2{\cdot}85}{4800} = 2126$ tons = the breaking weight of the tube*. Or, in case we take the ratio at 2·0 instead of 2·85, the breaking weight will then be 1492 tons.

Irrespective of the strength, and other proportions of the tube, I have computed the bottom plates, on the lower side, at 1 inch thick at the middle.

I do not however look upon them as plates of that thickness, but composed of two plates, each half an inch thick, with the transverse joints resting upon the solid plate on the one hand, and suspended from it on the other, as the case may be. This will give great uniformity of strength, when treble riveted, with a covering plate of the same thickness, well attached with tail-rivets to prevent curling at the ends, and perforating with as few holes as possible the solid plates, the same as per sketch at *a a* fig. 21.

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* Assuming the formula

$$W = \frac{adC}{l} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

to apply to the tubular beam, we have

$$C = \frac{lW}{ad} . \quad . \quad . \quad . \quad . \quad . \quad (2.),$$

where W = the breaking weight, l = the distance between the supports, a = the area of the bottom side, d = the whole depth of the beam, and C is a constant for all beams of the same form and material.

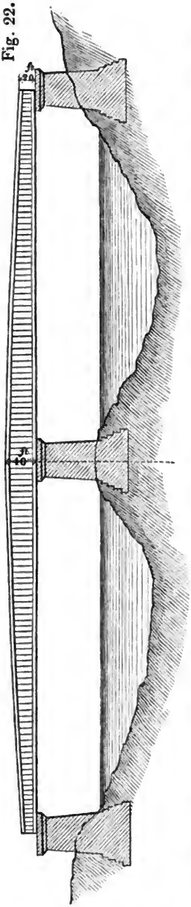
Taking the data afforded by experiment 19, we have, neglecting the weight of the tube,

$$l = 19 \times 12,\ W = \frac{22469}{112 \times 20}\ \text{tons},\ a = 2,\ \text{and}\ d = 15\tfrac{1}{3},$$

therefore by equation (2.),

$$C = \frac{19 \times 12 \times 22469}{2 \times 15\frac{1}{3} \times 112 \times 20} = 74,$$

Fig. 22.



This mode of crossing the joints and riveting would add greatly to the strength, and tend to give, as nearly as possible, the same degree of strength throughout all the parts of the tensile plates. On the top we shall have to attend more to the fitting, where the plates abut against each other to resist the crushing. These matters can however be more fully discussed when we come further into practical detail.

Will you take the trouble to look over this, and my former communications, which I send without revision? I shall be in London on Friday morning next, and will be glad to have as much time with you as you can spare. I will wait upon you at ten, and remain in the interim,

Yours faithfully,

William Fairbairn.

Robert Stephenson, Esq.

P.S.—Has it not occurred to you how it would suit to make the tube upon the equilibrium principle, taking the centre pier as the fulcrum, with equal lengths and equal weights on each side? Thus: the top in this case being made in the same proportion as before, or as 10 to 6 (fig. 22).

---

which is the value of the constant. Hence formula (1.) becomes

$$W = \frac{74ad}{l} \text{ tons}, \quad . \quad . \quad . \quad (3.)$$

where a is the number of square inches in the bottom side, and d and l must be expressed in the same linear unit.

Now if $a = 441{\cdot}5$, $d = 312$, and $l = 400 \times 12$, we have by formula (3.)

$$W = \frac{74 \times 441{\cdot}5 \times 312}{400 \times 12} = 2123 \text{ tons},$$

which corresponds, almost exactly, with the above results.

From the above letter it will be seen, that the formula is deduced from the results, obtained from the rectangular experimental tube with the corrugated top. It was the best and most satisfactory of the whole series, and gave a sectional area of the top and bottom sides in the ratio of about 5 to 3. This tube, if taken as the criterion of strength for those on the large scale, would give, according to the formula, a breaking weight of 2126 tons in the middle for the Conway tube, an important fact, which has since been verified by the tests of deflection, &c. applied to that structure.

Allusion is also made, in the letter, to the mode of riveting the lower parts of the bridge, that is, those parts which are subjected to a tensile strain, and an arrangement is pointed out, which, although it has not been entirely acted upon, yet gives the main security to structures of this kind.

By means of careful experiments this system was afterwards brought to much greater perfection, and the plates were arranged so as to equalize the strength of the joints and thus to secure them from injury.

The sketch, introduced in the postscript, is an idea which has since been applied in the parabolic form of the tubes for the Britannia Bridge. It gives an appearance of lightness to the tubes, and saves a considerable amount of material which does not contribute to the strength of the bridge. The next letter relates to a communication which Mr. Stephenson received from a gentleman who had taken great interest in the theory, as well as in the construction of the tubes. It also gives the proportions of the model tube, with its breaking weight, as calculated by the formula proposed in my last letter.

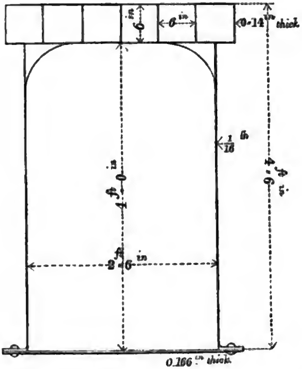
MY DEAR SIR, Manchester, April 24th, 1846.

I have read over the inclosed and retained a copy. I have not however come to the same conclusions, in every case, as the writer, although many of his deductions are pithy, and much to the purpose.

The more I consider the subject, and the more I become acquainted with the principle of construction and the nature of the material we have to deal with, the more strongly am I convinced of the absolute necessity of depending upon it, and it alone. It is true, that we cannot do without the chains in the erection of the bridge, but I *repudiate* their use afterwards, and I will cheerfully stake my own reputation, and probably yours also, which is more valuable, upon the strength and security of the structure. I however give way to your superior judgement, and submit to the chains as auxiliaries, till such time as increased knowledge and greater experience confirm the impressions I entertain.

By this post I have sent instructions to my son, at Millwall, desiring him to construct a model bridge, in every respect proportional to *one-sixth* of the bridge. It will be 75 feet between the supports, and of the following section, and thickness of plates, viz. the top to be composed of plates 0·14 inch thick, forming six rectangular tubes, each 6 inches square, and the bottom to be 0·166 inch thick, treble riveted at the transverse joints, and attached, as in sketch, to the sides, which are plates $\frac{1}{16}$th of an inch thick. This will give us an accurate experiment, and will still further develope (from the thinness of the plate) that property in the construction most to be guarded against, the puckering of the top side.

Fig. 23.



I have, however, guarded against this weakness by a defence of cellular cavities, represented in the sketch.

According to the formula I have deduced from the former experiments, this tube should carry 22·3 tons before it yields to pressure.

Taking the tube at 3 feet wide and 0·166 inch thick, the top being in a ratio of 10 : 6 to the bottom, we have

$$\frac{26 \times 5{\cdot}976 \times 54 \times 2{\cdot}4}{900} = 22{\cdot}3 \text{ tons.}$$

The breaking weight should therefore be 22 tons, which in the experiment we shall endeavour to prove.

I am, my dear Sir, always yours faithfully,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

P.S. The above experiment will, as far as I can judge, complete everything that is necessary for our practical guidance. Mr. Hodgkinson's calculations will follow, as confirmatory or otherwise, of what has been done. These experiments will establish the principle upon a sound and unerring basis, and will, I have no doubt, develope many new facts in connexion with what appears to me to be a perfectly new, and yet untried subject, viz. the efficiency and economy of malleable iron plates in the construction of bridges.

Notwithstanding the unbounded confidence which the experiments had implanted in my mind as to the ultimate success of the undertaking, it was shared by very few, and that only to a limited extent. Others, who were less sanguine, did not hesitate to condemn the whole proceeding as a wild, visionary project, which, according to their account, was unattainable either in theory or practice. Perhaps Mr. Stephenson was the only person, besides myself, who did not entertain these views; and although he had occasional misgivings, yet he nevertheless yielded to the conviction of every new fact, elicited by the experiments, and ultimately made up his mind to carry out the scheme without reserve, and to the fullest extent.

Serious delays had already occurred, and the urgent demands of the Directors could no longer be neglected. It was therefore decided to proceed at once with the experiments on a model tube, constructed on the scale which I had previously recommended, in the belief that sufficient data would thus be obtained to enable us to commence the construction of the Britannia Bridge, which had all along been the sole aim of my researches. This important model tube was accordingly prepared; a full description of its form and construction will be found in the sequel.

The breaking weight of this tube was computed from the formula already mentioned, and the area of the bottom being taken at 8·8 inches (the correct dimensions instead of 5·976, as given in the preceding statement), we have, by the new constant, as deduced from the experiments on the corrugated tube,

$$W = \frac{74 \times 8{\cdot}8 \times 54}{900} = 39{\cdot}07 \text{ tons*},$$

the breaking weight. Now the actual weight with which the tube broke was 35·5 tons, proving the accuracy of the formula, the comparatively trifling deficiency of 3½ tons being fully accounted for by the slight imperfection of construction observed in this tube.

Subsequent experiments sufficiently justified the general form of these conclusions, while an improved series of results determined the coefficient to range between 74 and 80. The latter number is now used as the coefficient in the formula for computing the strength of hollow girders and wrought iron tubes.

In conformity with the statements already adduced, orders were immediately given for the construction of the model tube, of dimensions proportional to one-sixth the size of those to be employed in the Britannia Bridge. The following letter, dated April 24th, will show the principle upon which it was constructed, with the form of the cells, joints and with other details relative to the experiments.

---

* See formula (3.), p. 69, where we have

$$a = 8{\cdot}8,\ d = 54, \text{ and } l = 75 \times 12 = 900.$$

Manchester, April 24th, 1846.

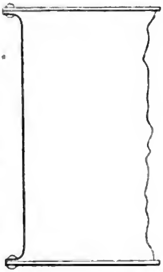
* * * * * * * * * * *

I have arranged with Mr. Stephenson to make another rectangular tube, exactly one-sixth the size and shape of the bridge for crossing the Menai Straits. I wish to have it made at Millwall, and the experiment to be conducted in London. This is not to interfere with those now in progress by Mr. Hodgkinson; and it will complete my report, which, I trust, if successful and satisfactory, will enable us to proceed with the bridge. You will observe that the top will be composed of six cellular tubes, each 6 inches square, and the bottom of two lines of plates, each 18 inches wide, and made, if possible, in three lengths on one side, and four on the other thus: see fig. 24, page 76.

I will, however, send you a correct drawing of this part with the mode of riveting, which we must be very particular about (see fig. 28).

The sides you may make as you think best, but they must be well riveted to the top and bottom, and the plates being very thin, they had better be turned on the edges thus (fig. 24): all the square tubes at the top to be firmly riveted to light angle-iron, as at *a*, *a*, *a*, &c. (see fig. 26).

Fig. 25.



After the tube is finished I will give you instructions about the weights being suspended, which I think will be best done from four suspension rods, and cross-bars resting upon the bottom, as at *b*, *b* (fig. 26); two holes to be cut through the sides of the tubes for this purpose.

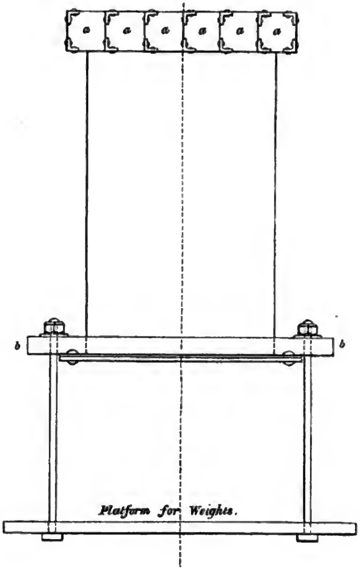
I think from this description you will be able to order the iron, which I wish to be of a good quality, and well put together. It will also enable James Graham to make preparations for supporting the tube. We shall want no beams, but will complete the experiments with dead weights.

Meanwhile, I am, yours affectionately,

Thomas Fairbairn, Esq. WILLIAM FAIRBAIRN.

P.S. The rectangular tube for experiment will be, as nearly as possible, one-sixth the size of the proposed bridge across the Menai Straits. It will be 78 feet long, and 75 feet between the supports, as shown in fig. 27. The thickness of the plates for the bottom (3 feet wide) to be 1·66 inch thick, and to be composed of two lines of longitudinal plates, with the joints crossed, as exhibited in figs. 24 and 28, and double riveted. The top to be composed of double plates 0·14 inch thick, and divided into six rectangular tubes, each 6 inches square, as shown at *a*, *a*, *a*, &c., fig. 26. The sides to be composed of $\frac{1}{16}$th-inch plates, attached to the top and bottom by light angle-iron; and three or four of the side plates, at each end, should be made strong, and thick, where they rest on the supports.

Fig. 26.



The next letter, dated April 25th, refers to the construction of both bridges, and the means to be employed for building the tubes and putting them together. At first it was contemplated to have them fitted and riveted, in parts, at the works of the contractors, and in that shape to forward them to their sites for

Fig. 24.

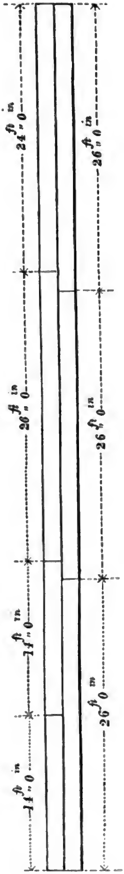


Fig. 27.

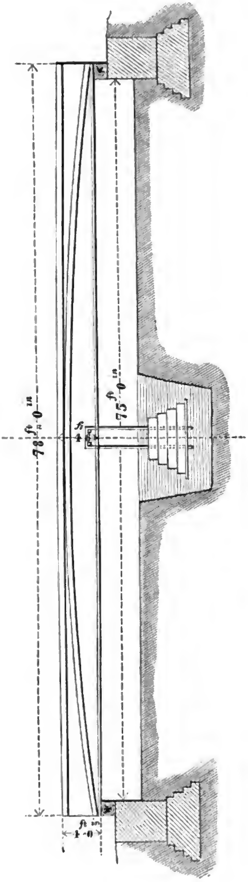
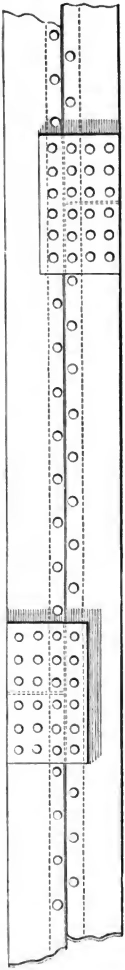


Fig. 28.



reconstruction. This arrangement, although at first feasible, was subsequently abandoned, when the mode of raising and erection came to be considered. Besides, it was found exceedingly troublesome and inconvenient to have the work divided, and the different operations progressing in distant parts of the country at one and the same time. Ultimately the whole of the works were concentrated at the Menai Straits and at Conway, in order that they might be placed under careful inspection, and that, at the same time, the requisite platforms and apparatus for constructing, floating and raising the tubes might be prepared.

My dear Sir, Manchester, April 25th, 1846.

As some months may elapse before arrangements can be made to commence the actual construction of the tubes for crossing the Menai Straits and the Conway, it may not be looked upon as premature if we begin to consider how this work is to be accomplished. There is a great deal to be done beforehand, and considerable preparation will be requisite in order to ensure despatch, as well as to give sufficient time, for the due and perfect execution of the work.

This is a consideration of some importance, and you having expressed a wish that I would undertake the direction and superintendence, I should be glad to have your opinion as to the best and most efficient mode of going about it. We must have a meeting on this subject before much can be done; and in order to prepare the way, I would suggest for consideration, whether the riveting and fitting should be done at the Straits, or in sections at different establishments in the country. I think the latter preferable, as it would relieve the Company of the expense of workshops, excepting, however, what may be necessary for joining the parts at the site of the bridge. I apprehend that the Company would prefer it done in this way. * * *

Yours sincerely,

Robert Stephenson, Esq. William Fairbairn.

The interval, from April 25th to May 11th, was chiefly occupied in preparing the drawings for the large tubes, in devising means for obtaining good material, and in various details connected with the dimensions of the plates, angle-iron, &c. to be used in the construction.

In the latter part of the work Mr. Clarke (Mr. Stephenson's assistant) along with Mr. Blair (my own assistant) rendered valuable service.

My dear Sir, Manchester, May 11th, 1846.

Since our last meeting on the subject of the bridge I have received Mr. Clarke's note, giving two separate statements of the proportions and thickness of the plates for the Conway Bridge. I confess I prefer the last proposal, 26 feet high, with the respective areas of the top and bottom sides in the ratio of 500 and 300 inches.

The vertical sides, in every case, I should make of $\frac{5}{8}$ths-inch plates, 2 feet wide, and the joints covered with ⊤ iron, thus: at every 2 feet as at A. I once thought that a thin plate in addition, as at B outside, might be useful; but on second consideration it would not be requisite, particularly if the sides are to be covered, or ornamented in the castellated style of the surrounding scenery. Two or three tiers of plates about 8 inches wide, and $\frac{5}{16}$ths of an inch thick, will, however, be necessary on the inside of the ⊤ iron, as shown at *a*, *a* and *b*, *b* in the annexed sketch. These strips will greatly tend to stiffen the sides for the support of the cellular top.

Fig. 29.

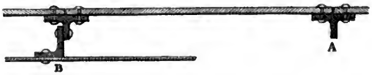
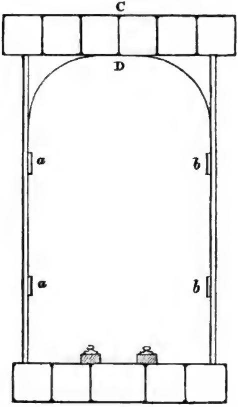


Fig. 30.



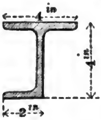
As we are to have single plates, and all the parts open, and easily approached for the purpose of cleaning, painting, &c., I would (with your permission) put six rectangular tubes or compartments on the top, of the same thickness as the crown plate; and the bottom plate D made thinner, so as in all to make up the required area of 500 inches. The bottom should be done in the same way, with five compartments, and the vertical division plates of the same thickness, viz. $\frac{5}{8}$ths of an inch, and the areas as before, 500 and 300 inches respectively. I apprehend this would be a good proportion, and better calculated for re-

sisting strain, the strong plates in every case being further from the centre or vertical axis of the beam.

I am, my dear Sir, yours faithfully,

Robert Stephenson, Esq. **WILLIAM FAIRBAIRN.**

Fig. 31.



P.S. If the ⊤ iron for the vertical sides could be rolled in this form, I should prefer it.

The distribution of the material, the size of the plates, and the methods to be pursued for putting them together, became matter of considerable importance; and much time and thought were devoted to the consideration of this part of the subject. The stiffness of the top, the strength of the bottom, and the retention of form in the sides, required the utmost attention; and no skill, however perfect, could have saved it from failure, unless governed by the facts and appearances presented in the experiments, which were on every occasion the unerring guide.

The great object to be attained was, that the resistance of every part of the tube should be so duly proportioned as to contribute its full share of resistance to strain when supporting itself, and the load which might be placed upon it. The sides, which appeared to have less work to perform than the top and bottom, were nevertheless an important part of the structure; and, owing to their great height, required some vertical support to keep them straight and free from "buckling." This was accomplished by the introduction of frames of ⊤ iron, which covered the joints of the vertical plates on both sides, and thus formed a line of pillars supporting the cellular top of the tube. These frames were also of great value in giving union and strength to every part of the structure, and, by their connexion with the top and bottom platforms, the requisite degree of solidity and stiffness was attained. We are indebted to Mr. Stephenson's suggestions for the arrangement of the ⊤ iron on the outside; according to the communication of May 11th, it was intended for one side only, but its application to both sides rendered the longitudinal strips, as shown in the sketch at *b*, *b*, &c., unnecessary (see fig. 30).

Prior to this period (May 1846) there had been several communications with Mr. Stephenson, as to the manner in which my services were to be made available in the progress of the works. At a meeting of the Board of Directors, on the 13th of May, as will be seen from the following extracts from the Minutes of the Board, I was appointed to superintend the construction of the bridges, in conjunction with Mr. Stephenson, on principles of perfect equality with him. This will sufficiently explain the motives which actuated me, when I resigned this appointment in May 1848. Had my position, as engineer to these particular works, been subordinate to that of Mr. Stephenson, I should most certainly have contented myself with the part I had taken in the preliminary experimental researches, and should have left to others the task of carrying into execution the views originally entertained by the engineer-in-chief, and which had been so essentially modified and improved by the results of my experiments. The duties which this appointment devolved upon me, not only required constant thought, but were at the same time of no ordinary responsibility: I willingly encountered both,—and I am persuaded that the correspondence from this period to the final completion of the Conway tubes, will show that both the designs and the construction were *left almost entirely* to my superintendence.

CHESTER AND HOLYHEAD RAILWAY.

Extract from Board Minutes of May 13, 1846.

Resolved,—That Mr. Fairbairn . . . . be appointed to superintend the construction and erection of the Conway and Britannia bridges, in conjunction with Mr. Stephenson.

2. That Mr. Fairbairn have, with Mr. Stephenson, the appointment of such persons as are necessary, subject to the powers of their dismissal by the Directors.

3. That Mr. Fairbairn furnish a list of the persons he requires, with the salaries that he proposes for all foremen or others above the class of workmen.

4. That advances of money be made on Mr. Fairbairn's requisition and certificates, which with the accounts or vouchers are to be furnished monthly.

5. That the Directors appoint a book-keeper at each spot, the Conway and the Menai.

GEORGE KING.

The works connected with the Conway Bridge may be said now to have been fairly commenced. Plans of the workshops, required for the construction of the tubes, were in progress; and much of the correspondence which follows necessarily relates to the details connected with this subject, to the appointment of inspectors, &c., and to inquiries respecting the best manufacturers of the plates and other material.

Mr. Stephenson at this time again listened to suggestions about chains; every care was taken to find out persons best qualified for executing these costly auxiliaries; and in the design of the masonry preparation was made for their application.

MY DEAR SIR, Manchester, June 11th, 1846.

I am going over to Wolverhampton, Stourbridge, and probably as far as Colebrook-dale on Monday. On Tuesday, or not later than Wednesday, I will have the pleasure of seeing you in George Street.

As stated in my hurried note of yesterday, I will bring the drawings of the workshops with me, and also such information as I can obtain about the manufacture of the chains. I shall consult some of the most intelligent iron manufacturers, such as Mr. Foster, Mr. Thorneycroft and the Colebrook-dale Company on that subject, and will use my best endeavours to have the links made without a weld, and otherwise constructed so as to have them fac-similes of each other.

I find I shall require the workshops about 500 feet long, and having now arranged all the drawings, specifications, tools, &c., I shall put the whole into the hands of a few respectable builders, in order to obtain tenders, and the time they will engage to deliver them complete at Conway and Bangor. The tools I have already ordered; and I have also put the crabs in hand, so that every thing may be ready by the time the buildings are finished. All that I shall now require, will be half an hour's inspection of the drawings of the workshops, before they are placed in the hands of the contractors. This, I hope, may be accomplished on Tuesday, and by the end of next week we should be in a condition to enter upon the contracts.

I am, my dear Sir, yours faithfully,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

DEAR SIR, Manchester, June 23rd, 1846.

Enclosed, you have tracings of the workshops for the Conway. That marked 1 shows a section of the building with the wings and diagonal braces, which I propose to fill up with at each end. At every 45 feet, we shall probably require mooring-rods on each side, extending from the top to piles driven into the ground to prevent the wind carrying away the whole structure; of this I will send you a sketch, as soon as I have seen the contractor on the subject.

In the meantime, you have in 2 a tracing of the platform we shall require, 488 feet long, for erecting the sheds upon. All that we shall require you to do, will be to drive the piles, and cut them level on the top-side ready for running the cross balk AA, and the flooring, which I think the contractor for the sheds had better furnish. I shall, however, be glad to have your opinion on these points, as also upon the description of slates it should be covered with. Boards would be lighter, but I apprehend slates will be fully as cheap.

I am, yours sincerely,

Alex. Ross, Esq., C.E. WILLIAM FAIRBAIRN.

In following out the experiments, and adapting them to the form and construction of the large tubes, several weighty considerations presented themselves, with reference principally to the strength of the cellular top, and whether the resisting powers of a tube 30 feet high, and 450 feet span, would follow the same law as that exhibited in small models of about only one-twentieth the size.

If we consider the magnitude of the structure, the extent of the span, the immense capital embarked, and the ruinous consequences which a failure would occasion; some idea may be formed of the great anxiety which these considerations entailed, and the necessity there existed for having always at hand a number of appliances, which gave promise of greater security, and held out prospects of success.

These feelings will account for the suggestion, in the annexed letter, of placing a wooden deck over the cells to stiffen the top, and still further to increase its powers of resistance to compression. The experiments on the large model-tube ultimately dissipated the fears which had been entertained, and enabled us, not only to dispense with the proposed wooden covering, but also with the double tier of cells, which, at that time, were proposed for giving increased security to the top of the tube.

My dear Sir, Manchester, June 26th, 1846.

I am devoting nearly the whole of my time to the bridges, as composed of frame-work, chains, &c.: I have made inquiries about the chains, and find that Messrs. Howard, Ravenhill and Co. have a patent, and are now rolling the links of suspension-chains as high as 6 cwt. each. This being the case, I have put myself in communication with them, and have arranged to visit their establishment next week, in order to inspect the process, and to see how far it will suit our purpose.

I sent off two sets of tracings of the workshops to Wales, one to Mr. Ross for the Conway, and the other for Mr. Foster at Bangor. They will enable them to prepare the platforms, and have all ready for the erection which I have stipulated with the contractors, that 200 feet in length should not be put up later than the 1st of September next, and the remainder to be finished by the beginning of November.

As soon as the tenders are received, I can either forward them to you or to the Secretary, Mr. King, as I presume the Directors will make the contracts.

I had not time to explain when last we met, my reasons for having the upper cells of the tubes 30 inches deep, and those below only 20 inches; nor yet did I explain my reasons for suggesting the timber deck, as shown in the last section. In the first instance, it must be borne in mind, that the drawing shows a section at the middle of the tube, where the cells are made deeper, in order to allow for the deflection; and at the piers, they will be of equal depth, namely, two tiers of 20 inches each way. Whether would it be better to have them equal throughout, retaining the rise or curvature on the top to allow for deflection, or would a square box 30 inches deep resist more pressure than one of 20 inches deep? I have to put this to the proof in our further experiments at Millwall, which my son informs me will be ready to be commenced upon about tomorrow week. I think we shall have to delay our discussion on those points until these experiments are completed. In the interval, I shall be glad to know if you could be disengaged on Monday or Tuesday, the 6th or 7th of July. One or either of those days I hope to have all ready, and I should like you to be present when the experiments are made; some new principle may present itself, and it is very advisable that you should be present to witness the results, whatever they may be.

As respects covering the top with wood, it may or may not be done. I expect it will not require such an addition, but it is well to be provided with a remedy in the event of any appearance of weakness in that part. Exclusive of the wooden top acting as an auxiliary to the top plates resisting a crushing force, I conceived it would be a good muffle, in conjunction with a thickness of vulcanized Indian-rubber below the rails, for deadening the canister rattling sound, which might ring the changes to every passing train. But of this we can talk hereafter. I am, my dear Sir, in hopes of seeing you next week,

Yours always,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

During the construction of the model tube, I took especial care that every despatch was used, consistent with the proper execution of the work. It was completed and ready for experiment by the end of the first week in June, and it yielded on its first test, with a weight of 35½ tons as before stated.

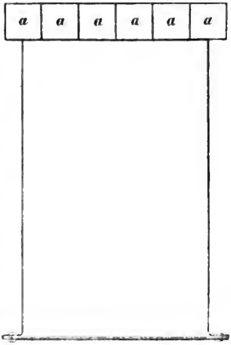
This tube was broken seven different times, and as often repaired. After each experiment, the injured and defective parts were cut out, and the tube restored to its original form, with plates of increased strength, or otherwise, as indicated by the nature and appearance of the fracture, or as circumstances might require.

Whilst progressing with the experiments, a question of considerable importance was frequently mooted by intelligent persons, as to the effect of the wind on the sides of the tube. Several professional gentlemen, and others conversant with the subject, entertained great doubts of its power to resist lateral strain; and in order to allay those fears, the tube was laid upon its side, and submitted to the test of experiment. That these experiments were satisfactory, will be seen on referring to experiment 35, in the Appendix, wherein the whole of the tests are carefully recorded. Mr. Hodgkinson was made acquainted with the result of the first experiment, as follows:—

My dear Sir, Polygon, Manchester, July 12th, 1846.

I should be wanting in candour, if I did not inform you that another experiment, upon a larger tube than any of the former, was made, at the request of the Directors of the Chester and Holyhead Railway Company, on Friday and Saturday last. That experiment appeared to be conclusive, as respects the strength and security of the bridges; and I have received instructions from Mr. Stephenson and the Directors to proceed immediately with the working drawings and execution of the tubes. You will, no doubt, consider this a premature decision, but the question with the Directors admitted of no delay, and I felt unwilling to disturb the experiments now in progress by any announcement which you might consider counter to those in your hands. On the contrary, it is distinctly understood

Fig. 32.



that your experiments are to proceed, and in such a way as you may deem most advisable for the further development of those laws which appear inseparable from the inquiry. The experiment of yesterday was made at Millwall, upon a tube, as near as possible, one-sixth the dimensions of the proposed bridge, as regards the span (75 feet), height, width, and thickness of plates. The section in the middle was as in fig. 32, with six cellular tubes, as *a*, *a*, *a*. It broke with 35½ tons by tearing the bottom plates at 20 inches from the centre of the tube.

I shall be glad to lay the whole before you, and remain, my dear Sir,

Yours very sincerely,

E. Hodgkinson, Esq. WILLIAM FAIRBAIRN.

Judging from Mr. Hodgkinson's reply to this communication, it appears that he had still been unable to satisfy himself, as to a correct formula for computing the strength of the large tubes; but the result of even the first experiment on the model tube was so satisfactory, that it warranted active proceedings without his assistance.

MY DEAR SIR, Salford, July 12th, 1846.

I feel, from your present note, that the decision of the Directors is already come to, and therefore I do not at present see what service my further efforts can be of. As I have no motives but honest ones, I feel that it will be necessary to see Mr. Stephenson and yourself before proceeding further: I know that the form will not be the best, but if it will do, it is nothing to me, except that I have been made an instrument of throwing away a deal of the Company's money through a deceptive understanding.

I am, my dear Sir, yours most truly,

William Fairbairn, Esq. E. HODGKINSON.

* * * * Manchester, July 13th, 1846.

After some gentlemen, who are interested in the last experiment, have seen it, you may then raise the top of the tube to its original position, cut out the fractured parts, put in plates a *little thicker*, rivet them upon the same principle as before, and restore the whole to its original shape. When this is done, we will lay it on its side, and ascertain its power to resist the action of the wind by a lateral strain. In this experiment we shall not injure the tube, but raise it again, and subsequently suspend a permanent weight of 32 tons from the centre as before. You will please to arrange all these matters, but I would not alter its present state for the next fortnight.

Yours affectionately,

Thomas Fairbairn, Esq. WILLIAM FAIRBAIRN.

MY DEAR SIR, Manchester, July 13th, 1846.

I sent a note to Mr. Hodgkinson immediately on my arrival yesterday, of which the enclosed is a copy. His reply this morning is,—

"I feel, from your present note, that the decision of the Directors is," &c. &c. (See letter of Mr. Hodgkinson, dated Salford, July 12, 1846.)

From the above you will see that an appeal will be made to you, and I am most anxious that my old and highly-esteemed friend should sustain no injury either in his feelings or reputation, which I am sure no one can appreciate more highly than myself. I could not, however, allow him to repudiate what had already been done, or yet to assume the position as a principal, when he was simply employed as an assistant. I think, under these circumstances, I have not acted improperly towards Mr. Hodgkinson, and shall ever be glad to receive any assistance which, in this important matter, he may choose to render. Should he, however, prove obstinate and refractory, we must then depend upon our own resources, which I think will not fail us in case of need.

I am, my dear Sir, faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

After this letter, Mr. Stephenson immediately wrote to Mr. Hodgkinson, and endeavoured to alleviate, if not to remove, the irritation under which he laboured. This communication, written on the following day, is as follows :—

MY DEAR SIR, London, July 14th, 1846.

Mr. Fairbairn has forwarded me a copy of your note to him, in reply to one addressed to you at my suggestion, before he left London a few days ago. I much regret the view you take of the matter, but in my position as engineer of the Holyhead Railway Company, and upon whom the responsibility of the Conway bridge being completed in time for opening that portion of the line (rests), you must perceive the difficulty I labour under. The Directors are pledged to the shareholders to have this portion of the line open by a certain period, and I am bound (even at the risk of not having arrived at the very best mode of distributing the material of the tube) to proceed; for what the consequence of delay, in a commercial point of view, after upwards of a million of money has been spent in finishing the works, not simply the interest, but the loss of income and these together, you will at once see, must become a very serious consideration both to the Directors and shareholders. Let me, therefore, beg of you to continue the investigation you have already begun, and I hope progressed with so far as to throw additional light upon the subject, and of which we can avail ourselves in the construction of the Britannia bridge. I hope to see you in Manchester in a few days: in the mean time, believe me,

Yours faithfully,

Eaton Hodgkinson, Esq., Manchester. ROBERT STEPHENSON.

Whilst the model tube was being repaired, after the first experiment, a correspondence of some interest took place, as to the surest and best means of moving and raising the tubes on to the piers. The following excellent suggestion was originally entertained by Mr. Stephenson: he proposed to erect suspension chains of a strength equal to the weight of the tubes, and from these chains to suspend a platform supporting a railway, over which the tubes should be drawn across the span to their final position. The more easily to accomplish this, it was proposed that the tubes should be built on platforms, at the required height, on each side of the Straits; and from these platforms, they were to be transferred or carried by wheels rolling along the rails, over the platform of the chain-bridge, to their resting-place upon the piers.

To this plan there appeared to be four objections:—1st, the depth of the tube (upwards of 30 feet) with its centre of gravity raised to a dangerous height; 2nd, the enormous weight required to be moved; 3rd, the danger of an oscillating motion of the suspension chains during the passage of the tube across the bridge; and lastly, the cost.

For the first of these objections, Mr. Stephenson provided a remedy by securing a sufficient width upon the rails; and for the third, he proposed the very ingenious expedient of having always the same weight upon the chains.

For example, supposing the tube to be 480 feet long and its weight 1500 tons, it was proposed to cover the suspended platform with loaded waggons of the same weight as the tube, and by one and the same process of haulage, to draw the same quantity of weight *off* the chains as would be drawn *upon* them by the forward motion of the tube. By this process the chains would have been kept in a uniform state of tension, and in every position and portion of the journey would have had the same load to sustain.

This was a clever, well-devised scheme, but rather hazardous in its execution, and attended with great expense, as I believe I am within bounds when I state, that the cost of such a catenary would not have been less than £150,000. Under all the circumstances, the plan of floating the tube to its position between the piers, and then raising it to its seat (as described in the following letter, July 15th), appeared to be the most eligible.

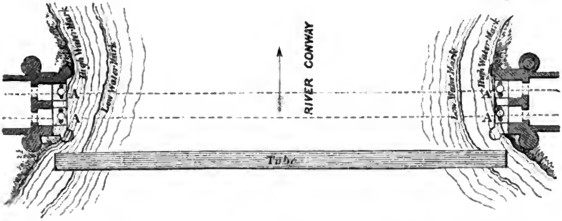
It will be noticed, that in order thus to launch the tube into the sea and to float it, both ends must have been closed, and all the parts made water-tight. Even then it would have required a considerable quantity of ballast, distributed over the bottom, to keep it vertical, and to retain it in that position until it was safely landed on the top of the hydraulic rams, by which it was proposed to raise it to its seat, and which are shown upon the piers, as prepared at A, A (fig. 33). But all these suggestions (in which I had the able assistance of Mr. Clarke) gave way to others of a more simple and effective character, as described in the succeeding letters.

My dear Sir, Manchester, July 15th, 1846.

After a careful and deliberate consultation with Mr. Clarke, we have come to a decision respecting the proportions and distribution of material in the tube. We have computed it to carry a dead weight in the centre of 2000 tons, or 4000 equally distributed over the bottom. Now this gives us a large margin for any contingency, and provided you agree with us, I would at once dispense with the chains, wheels and platform, and raise the tube into its place by a totally different process. The plan I have to suggest is this: to erect the tube upon the beach; close up the ends, launch it into the stream, and let it float with the tide alongside the piers of the bridge.

Having secured it in this position, float it into the two shelves of masonry, left open on purpose, as per sketch:

Fig. 33.



The two shelves A, A to be built a few feet above low water-mark, and upon each of them to be fixed two hydraulic cylinders. The tube would be floated at high water on to the top of the rams, and after the tide lowers the tube might be pumped up in an hour, say 10 feet, ready to receive the stonework to be built under. This being accomplished, the pumps are raised, and another 10 feet are made at each end until the whole height is attained.

As this can easily be done, and as we are satisfied we can do better without the chains than with them, either as a temporary or permanent attachment, I would suggest that you give the subject your serious consideration; as, in case we can accomplish this, a saving of time and one-half the cost of the bridge may be obtained. Besides the great amount of saving which would be effected to the shareholders, we are not deprived of the use of the chains, as, in case we should want them (which however I am satisfied we never shall), they can be attached afterwards, and probably better than before. I am looking for you here on Friday; and come, as before, direct to the Polygon. If Mr. Hodgkinson declines completing the experiments, which I hope for his own sake he will not do, I will continue them, and complete the whole to the best of my ability.

I am, my dear Sir, faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

MY DEAR SIR, London, July 17th, 1846.

I have had a note in reply to mine addressed to Mr. Hodgkinson, saying that I had put the thing in a different light. He adds, however, that the tubes he applied for had not been made, except two, and that when he is put in possession of them, he will continue the experiments he has undertaken, and he does not doubt being able to suggest an improved form of tube.

If you can accelerate the construction of the tubes Mr. H. requires I would do so immediately, in order to allay the irritation which he seems labouring under, I admit without sufficient cause; but let us try to keep matters square. I am fully aware that this is your intention and wish, I will not therefore say more on the subject. I would have been in Manchester ere this, but I have been unwell. * * * *

Yours faithfully,

William Fairbairn, Esq. ROBERT STEPHENSON.

Every day strengthened my dislike to the chains, and I never ceased to urge on Mr. Stephenson the serious waste of money, and absolute insecurity which their application would occasion.

Acting upon this conviction, the following letter was written, detailing the improved mode of floating and raising the tubes. The plan, herein recommended, was ultimately adopted, with certain modifications, in the position of the platform and the hydraulic pumps.

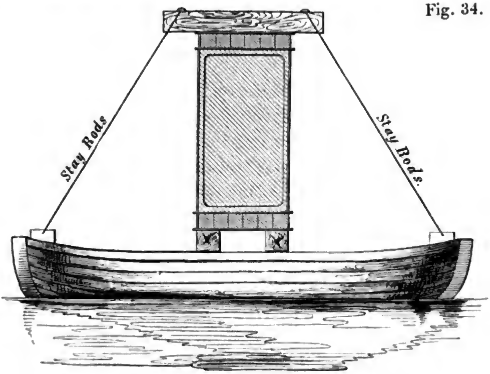
My dear Sir, Manchester, July 18th, 1846.

* * * * * * * * * *

I am quite aware of your desire to save as much of the Company's money as possible, and feeling assured of your willing cooperation in every sound and feasible scheme, I addressed a letter to you at Newcastle, some days since, roughly detailing the methods I proposed for raising the tubes. Since then I have gone more fully into the subject, and what I now wish you to consider is,—

First, to erect the workshops upon the beach, where a basin or excavation could be made to admit a number of barges, on which the tube should be constructed and put together. The entrance to the basin to be about 80 or 90 feet wide, with an embankemnt or coffer-dam across, so as to admit the tide after the tube is finished, and allow a passage for the barges, with the tube upon them broadside on, out of the basin into the tide-way as in sketch.

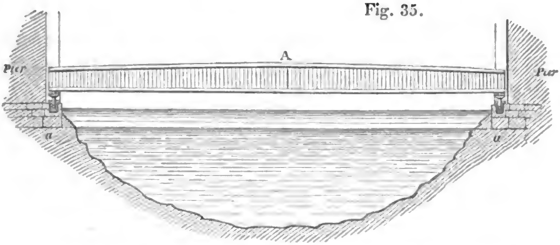
Fig. 34.



The raft of barges being once afloat would be towed down to the side of the bridge, and quietly floated into a shelf prepared on each of the piers on both sides to receive it. As the tide recedes the tube will remain suspended, and the barges may be removed from under it, in the manner shown in sketch below at A.

The tube having been lowered on to the shelf, formed as described above, two hydraulic rams, *a, a* on each side, are then put in motion by pumps, and the tube raised to a height of, say 10 feet.

Fig. 35.



After attaining this height, the masonry is built-in solid below to the bottom of the tube, which is thus supported till the water-cylinders are raised, when the same process is repeated until the desired height is attained. The tubes once raised to their permanent elevation, and properly tied with braces, it will then be time to consider how far chains are desirable. Should they be absolutely required, they can easily be attached after the tube is in its place; and should they not be wanted, there is then a clear saving of nearly £200,000 to the shareholders. This is a consideration of deep importance, and, conscious of your liberal views in matters of this kind, I am most anxious you should give the subject your deliberate consideration. Let us have your opinion and advice as soon as possible.

To get rid of the chains *will be a desideratum*; and I have made the tube of such strength, and intend putting it together upon such a principle, as will ensure its carrying a dead weight, equally distributed over its bottom surface, of 4000 tons. With a bridge of such powers, *what have we to fear? and why, in the name of truth and in the face of conclusive facts, should we hesitate to adopt measures, calculated not only to establish the principle as a triumph of art,* but what is of infinitely more importance to the shareholders, a saving of a large sum of money, nearly equal to half the cost of the bridge? I have been ably assisted by Mr. Clarke in all these contrivances, but in a matter of such importance we must have your sanction and support.

I am, my dear Sir, always faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

24 Great George Street, Westminster,
July 20th, 1846.

My dear Sir,

I should be delighted to get away, but there are two or three Boards of Directors threaten me with actions for damages, if I absent myself from London now that they have got their Committees in the Lords appointed.

I received your letter from Newcastle, and have turned over your proposal for another mode of erection, but the same plan was discussed at some length by myself and Mr. Ross for the Conway bridge. I proposed three pontoons upon which to place the tube, and then to float it into its place, exactly as proposed in your letter.

After much consideration, I felt that, although practicable, the risk of accident was so great, and the consequence of any miscarriage so serious, that I abandoned it, and resorted to the chains. The tidal current both at the Conway and at the Menai Straits is very rapid, and would render the management of such a mass as a tube and pontoons extremely difficult and precarious.

Under these impressions, I was led to adhere to the safest method notwithstanding the expense. In such an undertaking, I do not feel justified in running the least risk; but if I can see my way clearly, I shall not hesitate to modify my present views.

The objection which first seemed to be in the plan of raising the tube simultaneously with the masonry, was the delay which would take place with the masonry; because it is evident that we must stop proceedings at once, until the tubes are all completely finished. The tubes in this case would no doubt be useful auxiliaries for raising the stonework, but they, on the other hand, would much embarrass the process of setting. These however are not objections to the principle, and if safety can be attained, ought not to weigh at all. Now as to getting down to Manchester this week, I am afraid it is wholly impracticable; for I have three bills in the Lords, which I did not expect to come on so soon, and I dare not leave in spite of the Doctor, who is insisting upon my leaving town for some days, in order to get some *quietness*.

If therefore you and Mr. Clarke deem it essential to see me, I am under the necessity of asking you to come to town.

Yours sincerely,

William Fairbairn, Esq., Manchester. ROBERT STEPHENSON.

When two persons are engaged in an investigation, having the same object in view, it not unfrequently happens that they will simultaneously, and independently of each other, arrive at the same general conception. It appears from the preceding letter, that this was the case in reference to the plan of floating the Conway tube. When I communicated this plan to Mr. Stephenson, in my letter of the 18th of July, it was perfectly novel to my mind; but it appears that Mr. Stephenson had previously discussed the merits of the scheme, and had abandoned it, in consequence of the apparently greater security ensured by the plan of a loaded platform suspended by chains. The ulterior use of chains, no doubt, gave Mr. Stephenson a leaning to this latter plan; for it must be observed that, even at this advanced period, the plan of using chains as an auxiliary means of support, formed a leading idea of the structure in his mind.

The cellular form of the top of the Conway and Menai tubes constitutes one of the most remarkable features of the structure*. My earlier letters to Mr. Stephenson give a historical view of the series of inductions which led to this important conception. An unpleasant dispute, however, arose, at this time, with Mr. Hodgkinson, who asserted that he had suggested this form of structure. Now, fortunately for the cause of truth, Mr. Ross and Mr. Graham were present at the moment (September the 19th, 1844) when I first drew attention to the form of the intended model tube, and gave directions for the preparation of those with the corrugated tops (see figs. 9 and 13), and explained them to Mr. Hodgkinson. Moreover, the elliptical tubes with a single cell on the top, were made and experimented upon during Mr. Hodgkinson's *first visit*, and this clearly shows that the weakness indicated in the earlier experiments had induced me to look to a cellular structure, as that most likely to give increased powers of resistance. Mr. Hodgkinson was invited to Millwall for the express purpose of ascertaining the nature of the experiments which had been made, as well as of those which were in progress and under consideration, with the view of deducing some general formulæ. That form of tube, with the corrugated top especially, was sketched out for him, and also another form, recommended as preferable, with a series of cylindrical tubes or pipes, as given in the sketch*, and of which Mr. Hodgkinson seemed to approve.

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* This cellular form of structure is strikingly exemplified in the formation of the bones of animals, where great strength and lightness are combined. It also deserves notice, that Professor Moseley, in his excellent work on Engineering, page 557, seems to have entertained a similar idea in reference to the distribution of the material in wooden beams.

The letters from Mr. Ross and Mr. Graham, inserted in the Appendix, however, speak for themselves, and fully prove the groundlessness of the claim set up by Mr. Hodgkinson.

Mr. Stephenson was a good deal harassed about this time. A long parliamentary campaign, and the death of a near relative, rendered it advisable that he should have change of air and relaxation. He shortly afterwards left for the continent, and during his absence preparations were made to let the work in forming the tubes, and to erect the platforms, &c. for their construction.

My dear Sir, Menai Straits, August 14th, 1846.

Now that you have determined upon leaving us for some weeks, I am most anxious that everything should be settled before your departure. For this purpose I would beg leave to suggest, that you spend part of Saturday, and the whole of Sunday the 22nd with me at Manchester, there to examine the working drawings preparatory to letting the construction of the tubes before your departure.

A special Board might be called for that purpose, and in case Mr. Hodgkinson is not sufficiently advanced with his experiments, we could let them subject to such alterations and improvements as these experiments may ultimately render necessary and expedient.

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* See fig. 10, p. 17.

If you agree with me in these views, I shall at once proceed to the completion of the finished drawings, send out the circulars, and have everything settled before you leave. I am sure all this should be done, and I am most desirous, after you are gone, that you should have nothing on your mind to disturb the comfort and harmony of your enjoyment abroad. It is possible I may have to beg off for a few weeks after all this business is in train, as I want a little respite from other duties besides those of the bridges, in order to recruit an otherwise good constitution which received a severe shock two years since; but of this I will talk to you by and by. It shall not however interfere with the duties I owe to this important business, nor yet shall it lessen the comforts of your projected tour.

Captain Moorson informs me that it will be absolutely necessary to have you down here before your departure, as there are several persons you will require to see; and assuming this to be the case, it might probably suit your purpose (after the decisions are come to at Manchester) to proceed forward here, and finish all off at once. I will join you if necessary, or remain at home, as the case may be. At all events, I will meet you again in London, first to repeat the experiments on the model-tube, and crush the top, and afterwards to attend the Board and let the tubes. Tell me if this arrangement will suit, and believe me,

Yours faithfully,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

The following communication, August 15th, will explain the methods adopted for the construction of the stages or platforms on which the tubes were to be erected. At this time it was intended that the different contractors should construct and rivet the tubes in sections at their own premises; and that those parts should then be transported to the Straits and put together, and reconstructed on the stages prepared for that purpose, as shown in the following sketches (figs. 36 and 37).

This system of operation was afterwards modified, with the consent of the Contractors, and the works were proceeded with as nearly as possible to the respective sites of the bridges. A platform was afterwards erected for each of the large tubes, with preparations for placing the pontoons under them, when ready for floating, as represented in fig. 37.

MY DEAR SIR, Menai Straits, August 15th, 1846.

I am not only deprived of the benefit of your presence and assistance, but both Mr. Foster and Mr. Ross are from home, and out of the way, consequently Mr. Clarke and myself have the whole thing to ourselves. I have seen Captain Moorson this morning, and after a survey of the Straits last night, I think we shall finally decide as to the site for the erection and construction of the tubes. To the attainment of these objects, there are two methods which present themselves; the first of which is, to erect two of the tubes simultaneously alongside of each other, parallel to the beach; and the other to erect them simultaneously in a line upon a staked platform at some point between high and low water-mark. In the first arrangement, it would be necessary to have the workshops a fixture, 70 to 80 feet long, and to move the tube forward as it is put together out of the way, or, what is deemed preferable, to keep both tubes in a line, and move the workshops along the platform, as in the sketch.

In this case it would be desirable to have two moveable workshops with a crane to each, as shown in the side view and section at A A (fig. 36). These to be made as light as possible, and to be roofed with thin sheet iron, as a cover for the men to work under at any particular part of the tube.

These cranes might also be used for discharging the cargo of riveted plates as they come from the contractors, by forming a basin or wet dock at B, for the reception of the vessel at each end of the tubes. I have consulted with Mr. Foster and Mr. Clarke on this matter, and all parties being unanimous in their opinion as regards the efficiency of this mode of construction, we now only wait for your sanction in order to commence forthwith.

It is probable we may have to raise a rough framing along each side of the tubes, and move only the cover and top-part of the workshop with its attendant crab-hoist, which I apprehend will be preferable and less subject to accident from the wind.

Mr. Foster has just made his appearance, and having in conjunction with Captain Moorson made another survey of the Straits, to the south of the Britannia rock, we have determined upon fixing the platform alongside the margin between high and low water-mark, at a point on the east side, about 400 yards from the bridge.

In consequence of this determination, I have written to Messrs. Pauling to stop further proceedings, and requested them to make out a statement of the expenses they have already incurred in the preparations for the workshops: as soon as that document is received, I shall then be prepared to offer them another contract upon the arrangement I have now proposed. I trust all these proceedings will be satisfactory. It is important that no further time should be lost, and deeming it necessary to save yours as much as possible, I have come to these conclusions, which, if found correct, you will please to sanction by a note to Manchester, where I shall be after Saturday next.

I am, my dear Sir, yours faithfully,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN

Fig. 36.

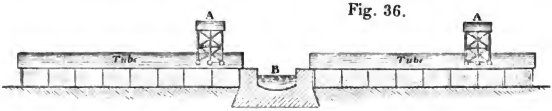
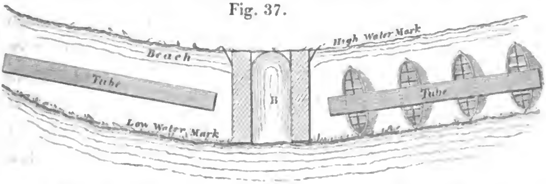


Fig. 37.



MY DEAR SIR, Newcastle-on-Tyne, August 18th, 1846.

I have made my arrangements to be in London next Saturday with a view of closing my business there for a month or five weeks. On Saturday evening I shall endeavour to leave by the mail for Manchester, in order that I may spend Monday with you to settle matters as to Conway. I am very much obliged for your attention to the arrangements at the Straits in my absence. I quite concur in all you have done, and hope you have put the shops, &c. in train. If you be in London on Saturday, I hope you will give me a call.

Yours sincerely,

William Fairbairn, Esq., Engineer, ROBERT STEPHENSON.
Manchester.

Shortly after the receipt of this letter, Mr. Stephenson left for the Continent, where he remained for six weeks. During that time preparations were in progress for the construction of the tubes and the wooden platforms at the Conway as well as the Menai Straits; and the different sites having been determined upon, no time was lost in commencing operations for carrying these objects into effect. Designs for the workshops were also prepared, and in four months from this time, the whole of the Carnarvon shore (extending from the site of the bridge to a distance of nearly a mile to the south) was covered with an active population of brick-makers, masons, carpenters, &c.

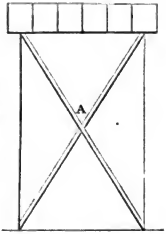
On the opposite shore the same activity prevailed, but upon a smaller scale, as the only works carried forward there were the abutments and piers for the support of the tubes on that side of the Straits. The manufacture of the tubes was confined to the eastern bank. Contrasting the beauty of the landscape and the murmurs of the waters, with the heating of rivets and the ringing of hammers, a highly interesting scene was presented to the view of the spectator. While the preparations for the scaffolding, &c. were going rapidly forward, the experiments at Millwall were not neglected. The tube had been repaired on two different occasions, and after having been crippled for want of stiffness on the sides, it was again restored and the experiments proceeded with as before. Immediately on Mr. Stephenson's return, I acquainted him with the results of the experiments, which had been made during his absence, in the following letter.

My dear Sir, Manchester, September 27th, 1846.

It was only yesterday that I heard of your return, I hope in good health and spirits. Mr. Clarke will inform you of the progress we have made with the various details of the tubes, and you will receive from him the working drawings, and, by this post, the specifications requisite to guide the parties in their tenders, which must be delivered at the office, Moorgate Street, on or before the 6th instant. I believe the drawings and specifications are all correct; but you will perhaps take the trouble of looking them over along with Mr. Clarke, and inform me how far they meet your approval.

In a former letter you will recollect the promise I made to give you an account of the last experiment made on the model-tube, at Millwall, on the 11th. Immediately after the experiment on the 31st of July, when the end of the tube gave way by twisting over with a weight of 94,710 lbs., or 42·3 tons, the model was again straightened, and the cause of the failure being a want of stiffness in the sides, those parts were repaired by the introduction of vertical pieces of 1½-inch angle iron, riveted at distances of 2 feet internally on each side. This was done, not for the purpose of increasing the strength of the sides, but to retain them in shape. An iron cross (as at A in the adjoining sketch) was also introduced at each end to keep the tube in its vertical position, in the same way as the large tube will be supported by the masonry of the towers at each end. Having restored the tube in this manner, we found its proportions as under:—

Fig. 38.



Area of the top	24·024 inches.
Area of the sides	9·000 inches.
Area of the bottom	12·800 inches.
Weight of the tube	5·15 tons.

On the 22nd of August, the tube was loaded with 77,527 lbs., which was left suspended for 19 hours, when the deflection increased from 3·25 to 3·5 inches. Part of this weight was then removed, and about 20 tons left suspended, from the 22nd of August to the 10th of September, when the deflections were again taken, and found to have increased only 0·25 of an inch. These appearances were highly satisfactory, and exceedingly interesting, as exhibiting a state of permanency in the resisting powers of the tube, evidently showing that all the parts were coming into play, and that in equilibrium with the load, upon the same principle as shown in my former experiments as to the effects of time on cast-iron bars. These are important facts, so far as they show a state of permanency in the resisting forces; and that more particularly when the molecules of the material have taken their respective positions to resist the destructive tendency of the load. Under such circumstances, the tube would carry (in the absence of a disturbing cause) the load *ad infinitum.*

After all the parts of the tube under strain were brought to bear, the experiment was resumed on the 10th instant, when the load was progressively increased from 77,527 lbs. to 126,138 lbs. or 56·3 tons, when it broke, by tearing the bottom asunder through the solid plates.

Now, if we compare the above results with the first experiment, we shall find, that by the simple addition of 5 cwt. more material, the bearing powers of the tube were raised from $35\frac{1}{2}$ to $56\frac{1}{4}$ tons; and even with this latter weight, the cellular top is yet too strong for the bottom, and I have no doubt, if the next experiment comes up to my expectation, that the previous experiments, as respects the ratio of 5 to 3, of the top to the bottom, will be fully confirmed. Assuming however the present to be the maximum, we should then have for the large tube the following results:—

Taking the area of the bottom of the model tube at 12·8 inches, the breaking weight 56·3 tons, depth 54 inches, and the distance between the supports 75 feet; we have (neglecting the weight of the tube), by Mr. Hodgkinson's formula, supposing the tube to be cast-iron,

$$\frac{26 \times 12{\cdot}8. \times 54}{900} = 19{\cdot}95,$$

or say 20 tons. Now, the same area for the bottom, when composed of plate iron, carried 56·3 tons. Hence

$$\frac{56{\cdot}3 \times 26}{20} = 73{\cdot}19$$

as the new constant for computing the strength of tubes of the same construction as the model. Taking therefore the new constant, we have for a tube six times the size in every respect, and six times the thickness of plates, or 450 feet span, 27 feet deep, and 460·8 inches area at the bottom,

$$\frac{73 \times 460{\cdot}8 \times 324}{5400} = 2018 \text{ tons},$$

the breaking weight, exclusive of the weight of the tube itself.

Let us now compare these results with Professor Airy's theory, that the strengths are as the squares, and the weights as the cubes; then we have half the weight of the tube added to the breaking weight, and this sum multiplied by the square of 6, or

$$\left(\frac{5{\cdot}15}{2} + 56{\cdot}3\right) \times 36 = 2119$$

tons as the breaking weight, including its own weight. Again,

$$6^3 \times 5{\cdot}15 = 1112{\cdot}4$$

tons as the weight of the tube. If therefore 2119 tons be the maximum load that the tube will bear, and 1112 tons the weight of the tube itself, it then follows that 2119−556=1563 tons, is the load the tube will carry exclusive of its own weight; or, by the formula when the weight of the tube is added to the breaking weight, the constant will in round numbers be from 73 to 80. Hence

$$\frac{80 \times 460 \times 324}{5400} - 556 = 2208 - 556 = 1652 \text{ tons*},$$

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* Let W = the total breaking weight of the experimental tube, w = the half weight of the tube, and L = the weight of the breaking load; then $W = L + w$. Substituting this value of W in equation (1.) page 68, we have

$$L + w = \frac{adC}{l} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

$$\therefore \; C = \frac{l(L+w)}{ad} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

Taking $l = 75 \times 12$, $a = 12{\cdot}8$, $d = 54$, $L = 56{\cdot}3$, and $w = \frac{5{\cdot}15}{2} = 2{\cdot}575$, as determined by the foregoing experiment, we find

$$C = \frac{75 \times 12(56{\cdot}3 + 2{\cdot}575)}{12{\cdot}8 \times 54} = 76{\cdot}66,$$

which is the value of the constant. Substituting this in equation (1.), we find

$$L_{,} = \frac{76{\cdot}66 a_{,} d_{,}}{l_{,}} - w_{,} \text{ tons,} \quad . \quad . \quad . \quad . \quad (3.)$$

Taking C=80, then we have

$$L_{,} = \frac{80 a_{,} d_{,}}{l_{,}} - w_{,} \text{ tons,} \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

which is the general expression for the breaking load in tons, of any tube similar in form to the one from which the constant C has been determined. Taking $l_{,} = 462$, $d_{,} = 31$, $a_{,} = 460$, and $w_{,} = \frac{1200}{2} = 600$, then by formula (4.)

$$L_{,} = \frac{80 \times 460 \times 31}{462} - 600 = 1869 \text{ tons,}$$

which is the result obtained in the letter.

When the large tube is in all respects similar to the experimental one, we can determine $w_{,}$ in terms of w, thus we have, from the property of similar solids,

$$w : w_{,} :: l^3 : l_{,}^3$$

$$\therefore \; w_{,} = \left(\frac{l_{,}}{l}\right)^3 . w \text{ tons.} \quad . \quad . \quad . \quad . \quad (5.)$$

The expressions (4.) and (5.) are, substantially, the formulæ by which I have the breaking weight (which is rather a higher value than the one given above). Assuming that this is correct, which I am persuaded is not far from the mark, we should then have, for one of the Britannia tubes, 462 feet span, 31 feet deep, and 460 inches area in the bottom,

$$\frac{80 \times 460 \times 372}{5544} = 2469 \text{ tons};$$

and supposing the tube to weigh 1200 tons, we should have for the breaking weight $2469 - 600 = 1869$ tons, the load it would carry in the middle, exclusive of its own weight. If it therefore carries 1869 tons in the middle, exclusive of its own weight, and the greatest load that can

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made all my calculations. Taking

$l = 75$, $w = \frac{5 \cdot 15}{2}$ tons, $l_{,} = 6 \times 75 = 450$, $d_{,} = 6 \times \frac{54}{12} = 27$, and $a_{,} = 6^2 \times 12 \cdot 8 = 460 \cdot 8$, we have from equation (5.)

$$w_{,} = \left(\frac{450}{75}\right)^3 \times \frac{5 \cdot 15}{2} = 556 \text{ tons; and from equation (3.)}$$

$$L_{,} = \frac{76 \cdot 65 \times 460 \cdot 8 \times 27}{450} - 556 = 1563 \text{ tons.}$$

Taking the value of the constant $C = 80$, we should find from formula (4), $L_{,} = 1656$ tons, as determined in the letter.

It is desirable that we should have a formula containing, in one expression, the essential data of the problem.

Assuming
$$W = \frac{adC}{l},$$

$$\therefore \quad W_{,} = \frac{a_{,}d_{,}C}{l_{,}},$$

hence by division we have

$$\frac{W_{,}}{W} = \frac{a_{,}d_{,}l}{adl_{,}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

But since the solids are supposed to be similar, we have

$$a : a_{,} \; :: l^2 : l_{,}^2,$$

and
$$d : d_{,} \; :: l : l_{,},$$

$$\therefore \quad ad : a_{,}d_{,} :: l^3 : l_{,}^3,$$

$$\therefore \quad \frac{a_{,}d_{,}}{ad} = \frac{l_{,}^3}{l^3};$$

substituting in equation (6.),

$$\frac{W_{,}}{W} = \frac{l_{,}^3}{l^3} \cdot \frac{l}{l_{,}} = \frac{l_{,}^2}{l^2}.$$

$$\therefore \quad W_{,} = \frac{l_{,}^2}{l^2} \cdot W. \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

be put upon it be 1 ton in every lineal foot, it is evident that the load will be in the ratio of the strength as 462 : 3738, or as 1 to 8. It is true that the tube must always be subject to a permanent load of half its own weight and the permanent way, which may equal 650 tons; but this taken from 2469 tons still leaves 1819 tons, or 3638 tons distributed equally over the surface of the lower part of the tube, which is eight times the greatest load that ever can be brought upon it.

I have gone further into the investigation than I at first intended, but I feel anxious to put you in possession of all these facts, and I shall be glad if your conclusions harmonize with those I have given above. Should they differ, I should be glad to know wherein that difference consists, and I shall be most happy to receive from the facts deductions more conclusive than my own.

The meeting of the British Association, at Southampton*, appeared a fitting opportunity for ascertaining the opinions of the different "savans"

Substituting $L+w$ for W, and $L_{,}+w_{,}$ for $W_{,}$, we have

$$L_{,}+w_{,}=\frac{l_{,}^2}{l^2}(L+w),$$

$$\therefore\quad L_{,}=\frac{l_{,}^2}{l^2}(L+w)-w_{,};$$

but by equation (5.) we have

$$w_{,}=\left(\frac{l_{,}}{l}\right)^3\cdot w,$$

$$\therefore\quad L_{,}=\left(\frac{l_{,}}{l}\right)^2(L+w)-\left(\frac{l_{,}}{l}\right)^3\cdot w$$

$$=\left(\frac{l_{,}}{l}\right)^2\left\{L-\frac{l_{,}-l}{l}\cdot w\right\}\text{tons},\quad\ldots\quad(8.)$$

which expresses the breaking load of a tube $l_{,}$ feet long, and in all respects *similar* to an experimental tube, whose length is l feet, weight w tons, and breaking load L tons. This formula is convenient for calculation.

Taking $l=75$, $L=56{\cdot}3$, $w=\frac{5{\cdot}15}{2}$, and $l_{,}=450$, we have

$$L_{,}=\left(\frac{450}{75}\right)^2\left\{56{\cdot}3-\frac{450-75}{75}\times\frac{5{\cdot}15}{2}\right\}=1563\text{ tons},$$

which is the same result as that obtained from formula (3.).

* Some time previous to the meeting, Mr. Hodgkinson, in order to enforce his claims relative to the cellular structure of the top of the tubes, intimated his intention of making these claims public; this determination led to the discussion which took place at the meeting of the British Association.

as to the results of our experiments; conceiving that in case we did not receive assistance in the inquiry, we should at all events hear the objections which different men with different minds might urge against them. These objections were not so numerous as I expected; and reverting to the animated and interesting discussion which took place, I should draw the conclusion, that the opinions of theoretical men of science, as well as the public at large, are in our favour.

Before coming to a decision as to giving the meeting a short abstract of results, I sent a note to Mr. Hodgkinson, intimating my intention, and offering him the opportunity to make any statement he chose, rather than forestall him in a position to which I had no doubt he would claim a precedence. To that note I received no reply; but immediately after I had given an account of the results obtained at Millwall, he read a paper claiming the whole merit to himself, and that everything was done at his suggestion.

* * * * * * * * * *

Even now it is in vain for me to tell my friend that I have undeniable proofs of the cellular top having been talked of and decided upon before he ever saw the experiments. He believes nothing, and will listen to nothing; I must therefore leave it to himself, as I have no doubt you and others will do me justice in this and every other claim to which I am justly entitled.

I am, my dear Sir, faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

The experiments, detailed in the preceding letter, are probably amongst the most interesting of the whole series. The letter explains the high state of perfection to which the experiments had been carried, as regards the properties of wrought iron tubes, and by calculations, based upon these experiments, fully demonstrates that the maximum or ultimate strength of the tube proposed, for the construction of the Menai Bridge, is far more than sufficient to carry the greatest load which can ever be placed upon it, without the aid of auxiliary chains. Moreover, a higher value was derived for the constant in my formula for the strength of tubes, than had been assigned by the previous experiments. Although this formula did not entirely agree with Mr. Stephenson's, yet it has been found sufficiently accurate for every practical purpose, and is now much in use for computing the strength of wrought iron hollow tubular girders.

The other parts of the letter have reference to the meeting of the British Association for the Advancement of Science, held at Southampton, at which a brief statement of the results of the experiments was given.

The next two letters are explanatory of the misunderstanding which continued to exist between Mr. Hodgkinson and myself, and the debate which ensued in the Mechanical Section of the British Association at Southampton. The discussion was much to be regretted for several reasons, which however have no particular interest in relation to the present subject of inquiry.

My dear Sir, Shrewsbury, October 7th, 1846.

Although I have not replied to your letters I have attentively perused them, and shall be prepared to discuss the calculations when we meet. Your notion of the formula, as applicable to tubes, does not quite tally with mine.

I conclude you intend being in town on Saturday, as I understand the tube will be ready for another trial at Millwall.

I hear an unsatisfactory account of the exhibition at Southampton on all sides; indeed when I first heard of it I was both surprised and disappointed; for I felt that either the Board or myself should have been consulted on a matter in which the interests of a large proprietary are concerned. As the question now stands, a doubt is thrown over the whole thing by what fell from Mr. Hodgkinson, which I deeply regret; but on this subject I will talk to you when we meet.

On Saturday morning I shall be engaged, but in the afternoon I will endeavour to see you at Millwall, and if I do not, pray arrange that we may meet somewhere in the evening. I wrote to Mr. H. by this post, expressing a wish that he may be at Millwall on Saturday, which I trust will be convenient to you and him.

Yours faithfully,

W. Fairbairn, Esq. ROBERT STEPHENSON.

My dear Sir, Manchester, October 9th, 1846.

I believe you are right, and I stand chargeable with precipitancy in having allowed the subject of the experiments to come at all before the sections at Southampton. My object however was threefold: first, to allay all doubts about the undertaking, by recording the many satisfactory experiments on the model tube; secondly, to obtain some further theoretical knowledge on a subject of such importance from the leading men present at the meeting; and lastly, to put an end to the unpleasant position in which I have all along been placed with Mr. Hodgkinson.

Again, I was strongly urged by several parties to bring it forward, and I mentioned the subject to one of the Directors: your absence prevented the possibility of consultation, and was the sole cause of my venturing upon such a step.

It is however now over, and I hope without any injurious effect. We must be more careful in future, and being in a great measure inexperienced in railway matters, nothing shall take place on my part without your privacy and consent.

As respects the misunderstanding between Mr. Hodgkinson and myself, that is now all settled, and I shall cheerfully wait the result of his experiments, and make any alterations in the form or construction of the top, as the facts may determine.

I must not however disguise from you the great expense which these experiments, as well as my own, will entail upon the Company, and I am most anxious, as soon as you are satisfied and we see our way clearly, to put a stop to them. I intend the one now forthcoming at Millwall (if satisfactory) to be the last, and I hope we may also be able to terminate those here upon the same principle.

So far as regards myself, I am perfectly satisfied; and exclusive of all theoretical inquiry, I have no hesitation, from the experimental facts before us, to give an unqualified assurance that the work can be done at a moderate expense, and that successfully. If you concur with me in this opinion, I will set my shoulder to the wheel, complete the work, and then ask the opinion of the mathematicians after the whole is finished and at work. The present discussions on the subject in the different journals is mere moonshine. *I will stick to the facts before the figures*; and although I am far from despising theoretical knowledge, I would nevertheless infinitely prefer solid facts, and my own judgement in reasoning from such data, for the construction and consummation of such a work. All along I have unreservedly spoken my mind, and put you in possession of every circumstance connected with this inquiry.

All these facts have now so firmly established my opinion of the practicability of this undertaking, that in case I stood alone, with only an occasional consultation in George Street, *I could build the bridge.*

The tube will not be ready before Tuesday or Wednesday next. I believe it rained incessantly in London last week, which has in a great measure retarded the progress of the work. I am however writing this evening to request that they will, without fail, be ready for us on Tuesday morning, when I shall call upon you, and I hope that you will keep yourself disengaged for the purpose.

I am, my dear Sir, always faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

P.S. I will bring Mr. Hodgkinson with me.

It appears from the following letter, that Mr. Stephenson had the subject of the experiments and the proposed construction of the tubes under consideration.

In this communication he obviously clings, with considerable tenacity, to the notion of bringing the upper side of the tube into a state of tension. This was however an exceedingly difficult problem, which, however, we attempted to solve in experiment 18*. In this experiment it became evident, that the tube, owing to its rigidity and the peculiar nature of the strain, was resolved into a simple rigid girder, resisting severe transverse strain in the usual way, viz. by extension on the lower, and compression on the upper side. Every previous and subsequent experiment gave evidence of these facts; and thus every attempt to meet the views of Mr. Stephenson in this respect proved abortive.

These facts having been clearly established, the advantages (whatever they might be) of bringing the top of the girder into a state of tension were with common consent abandoned; and in making out the calculations, the tubes were taken as a common girder or beam, and that without reference to the attainment of tension on the upper side caused by the deflection of the tubes over the piers.

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* See Appendix.

The top and bottom sides of the tubes for the Britannia Bridge were intended to be parallel; but on further consideration, they were altered to their present form, with a rise or camber of 7 feet on the centre pier, and tapering with a slight curvature from a depth of 30 feet in the middle to 23 feet at each end*.

It will be observed, that the square cells, in conjunction with each other, as exhibited in the model tube, gave indications of great strength: when treated separately as pillars they were weak, and according to Mr. Hodgkinson's experiments the cylindrical forms were unquestionably superior; but taking the square cells in conjunction,—as represented in the model, and in the large tubes themselves, with strong angle-iron riveted along the angles of each cell,—a perfectly rigid piece of framework of great power is presented to the force of compression.

This principle of construction affords every security in the resisting powers of the top of the tube, and is, in other respects, infinitely superior to the circular cells.

The method of lifting the tube had been under consideration since the time when it was proposed to float it, and then raise it to its place on the piers by means of hydraulic power†. Various forms and positions were, from time to time, assigned to the pumps, all of which terminated in fixing them on strong cast-iron girders, built into the towers, with strong chains attached to the cross head of the rams above, and to cast-iron frames and four cast-iron girders, fixed transversely to the ends on the inside of the tube below.

The tube was raised to its place by a series of lifts of 6 feet each. In effecting this object, it was necessary to retain it suspended at each successive lift, till the hydraulic ram was lowered for the next elevation, and so on till the required height was attained. This was done, on a very simple principle, by the introduction of "clips" or "clambs" which were made to slide under the shoulders of the joints of the chains, which resting upon a solid cast-iron frame attached to the girders, supported the tube till the pumps were lowered; and by a similar arrangement of clips on the cross head of the ram, the raising process was continued as before. From this description there will be no difficulty in perceiving how the tubes were raised into their places. All the machinery was computed to bear three times the weight of the tube (equal to a load of 4500 tons); and having a steam-engine at each end for working the pumps, the process became at once easy and perfectly secure.

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* See Plate, which exhibits a side view of one-half of the Britannia tubes.

† See Plate showing the machinery and apparatus for raising the tube.

The successive suggestions made to prevent the twisting of the tubes were of great value, as previous to this time there had been no experiments made having a special reference to this point, excepting those on the model tube, on which perfect reliance in this respect could not be placed. In the end, however, great strength was attained from the stiffness of the connexion between the top and bottom cells of the tube, which completely set at rest all fears on this head*.

My dear Sir, London, October 25th, 1846.

I spent some time yesterday with Mr. Clarke, discussing the subjects of your last letter, and I shall again do so tomorrow, previous to his leaving for Manchester, which he will do in the evening, either by the express or mail train.

If the upper part of the tube, resting on the piers, is to be brought into a state of tension (which I feel certain it ought to be), then I doubt the propriety of lessening the depth of the tube at these points, at least to any considerable extent. It appears to me, that the tube should be parallel top and bottom, only giving such a camber both top and bottom, as would allow of the tube becoming quite level with its own weight.

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* See Plate representing a sectional view of the Conway tube.

On this point however I will go more into detail tomorrow, and Clarke will communicate my ideas on his arrival.

The drawing with the square cells I have examined, and think an improvement; and looking at the results of the last experiments at Millwall, the rectangular cells appear to be quite safe, and therefore render the circular arrangement of less consequence; but should the last experiments of Mr. Hodgkinson show a very decided superiority in the circular forms, then I would advise you to consider the best mode of constructing the circular cells, when I will meet you personally to decide which is to be acted upon. This I shall be able to do tomorrow week, and at the same time settle how we are to carry out the contract with Evans.

This will require some management, for it is evident that everything he does must be first sanctioned by you and myself, not only in his proceedings for constructing the tube, but especially in moving the tube into its place: this I cannot leave to him as his contract contemplated; but the modification in the arrangement I do not apprehend will give us any trouble.

I am sincerely glad that your son and Ditchburn have succeeded in arranging with the Company. We must put the whole of the Britannia into their hands, as I am sure the others are unequal to the thing. We must visit both their establishments when I come down to Manchester to decide the other matters.

The method of lifting requires more consideration than it has had. The pumps in the tubes have some advantages in the steadiness, over the plan of placing them on the tops of the high walls between which the tubes are finally to rest.

If the tubes are partly suspended by the top of the piers, much of the necessity for strengthening the sides by cast-iron pillars is removed, but I lean to the adoption of both. The liability of the tubes to twist presses much on my mind: the strength I have no fears about, but the twisting tendency, owing to the height being so much greater than the width, will not be inconsiderable, and demands *further reflection.*

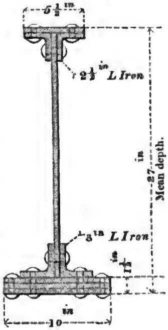
Believe me, yours faithfully,

William Fairbairn, Esq., Manchester. ROBERT STEPHENSON.

Mr. Stephenson's letter of the 26th of October refers to a report then in circulation, that Mr. Rendal had suggested the idea of a wrought iron tube for carrying the railway across the Menai Straits. To this report Mr. Stephenson very properly gave a direct contradiction, and after such an assurance, the question, as to the origin of the idea, could not for a moment be entertained.

In the latter part of the same letter Mr. Stephenson states, that "this was not the first time I had the idea of employing wrought iron tubular bridges; for three years ago, or thereabouts, I had erected at Ware, on the Northern and Eastern Railway, a cellular platform of wrought iron; . . . it was in fact I believe a counterpart of the proposed top of the Britannia Bridge." Now as this statement has been frequently repeated, since the letter was written, I feel myself called upon to show that Mr. Stephenson has no claim to originality in this bridge, and that it has no resemblance whatever either in principle or construction to the Conway or Britannia tubes. On the contrary, the bridge in question is constructed upon the principle of the common cast-iron girder bridge, each separate beam being formed of wrought-iron plates connected together by angle-irons, as shown in the annexed sketch. This form of wrought-iron girder had been long in use before the erection of the Ware Bridge; and it is defective, as well in principle as in construction: the great body of the material is not in the top flanches, as it ought to be, in order to attain the section of greatest strength*. Besides, it is impossible to trace any analogy between a combination of this form of beam and a

Fig. 39.



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* In experiments 14, 15 and 16 (see Appendix and p. 40 in the Report), it is clearly shown that the top flanche of a wrought iron girder, if made solid, should be more than twice the area of the bottom flanche. Now it appears, from the above sketch, that the top flanche is to the bottom as 4 : 15 nearly :

tubular girder with a *cellular top.* The beams in the Ware Bridge do not offer a united resistance to strain, in the manner which beams with a cellular structure do; on the contrary, each beam has its own distinct part of the load to carry, and that very imperfectly for want of a due proportion in the top and bottom flanches. An immense tube, upwards of a thousand tons in weight, suspended in the air, and through which railway trains should pass, was a magnificent conception; surely Mr. Stephenson does injustice to his original idea, as well as to the spirit with which it was realized, when he couples it with so imperfect a structure. While I freely award to Mr. Stephenson the honour of this conception, I claim for myself the credit of having rendered it practicable.

MY DEAR SIR, London, October 26th, 1846.

* * * * * * * * * *

I fear I am in a fair way of losing even a little credit for the introduction of tubular bridges, from what you say in your note in reference to Mr. Rendel. I am really desirous of avoiding a controversy on this matter, but I perceive it will be inevitable if such malicious and selfish rumours are set afloat. Mr. Rendel had nothing to do whatever with the original idea: it was entirely my own. Mr. Rendel was called in to give evidence before a Parliamentary Committee, to support some statements which I had made before the Committee, and which rather startled them, but I then had made up my mind on the subject. But this was not the first time I had the idea of employing wrought iron tubular bridges; for three years ago, or thereabouts, I had erected at Ware, on the Northern and Eastern Railway, a cellular platform of wrought iron where we were very much pinched for height; it was in fact I believe a

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an exceedingly defective structure. If this beam were turned upside down it would carry more than double the weight. From the defective principle upon which the bridge is constructed, it is evident that Mr. Stephenson was not then acquainted with the proper form of wrought-iron girder bridges. Nor is this surprising, as no experimental facts were at that time in existence to show the difference between the two resisting forces of compression and extension of wrought-iron beams.

counterpart of the proposed top of the Britannia Bridge. Fox, Henderson and Company made the bridge, and I will get the drawings raked out to show you.

I am, dear Sir, yours faithfully,

William Fairbairn, Esq. ROBERT STEPHENSON.

In the following reply to Mr. Stephenson's letter, after expressing my conviction relative to his claim on the original idea of tubular bridges, I proceed to notice the long controverted question of bringing the top of the tube into a condition of tensile strain; and other important points connected with the designs for the bridge, such as,—the proportional depths for the tubes at different points of their lengths,—the comparative eligibility of the square and cylindrical cells,—and the means to be employed in order to prevent the tubes becoming twisted. The fears, however, entertained on this latter point are shown to be groundless, provided certain precautions of construction be adopted.

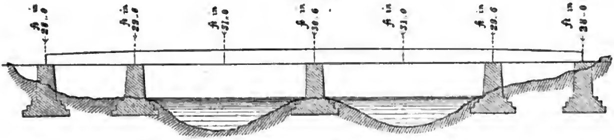
MY DEAR SIR, Manchester, October 27th, 1846.

I am much obliged by your letter of yesterday, and particularly for that part of it which relates to the *original idea* of the bridge. I was sure it was yours in every respect; but there is nothing new, or likely to turn out valuable, but there immediately start up a hundred claimants. We are all subject to this species of mental encroachment; but in your case everything is now clear, and no person can possibly establish a prior claim.

At all times, you may rest assured of my best efforts in supporting the claim, to which you are so justly entitled.

I am glad to find, you are likely to be so soon down in this part of the country. We shall require you here a whole day, and I will write to King, requesting him to send down drafts of the different contracts, in order that we may go over them together, previous to their being signed. We shall further require some discussion on the cellular top, and the camber necessary to be given to the Britannia tubes. At present they have 18 inches, which you will not find too much, in such a great length of tube and of such great weight as that across the Straits. I could wish the top, in its utmost deflection, at all times to be 6 inches above the horizontal line of pressure. I think the only difference between us is 6 inches: you require it level with its own weight; and in order to meet

Fig. 40.



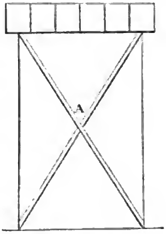
you in this respect, I would submit that the depths of the tube be as above.

On the above principle, the upper side at the piers might be brought into a state of tension, and the ultimate depth of the tube probably reduced to 30 instead of 31 feet. We shall however finally decide upon this, when we have the pleasure of seeing you here.

The circular cells are no doubt preferable to the squares, but I doubt the propriety of their application. The rectangles or squares may not be so strong, but they are much easier of execution; and, practically speaking, they are infinitely preferable. Besides, the rectangular cells, as constructed in the model tube, are of the first order: as yet, we have never been able to crush them; and the great convenience of flat straight plates with easy access to any part, is a consideration we must on no account lose sight of.

I am not afraid of anything like twist in the large tube, as the bending and riveting of the T iron to the top and bottom platforms will give every security in that respect. We shall, however, use every precaution by the introduction of temporary cross-frames, till the tubes are erected and in their places, thus, as at A: I think we shall not have much difficulty about the pumps. Mr. Clarke and myself will have all ready for you when you come down. Let us hear when you can come, and if you can leave by the Express; come direct to the Polygon, where you will find everything prepared for your reception.

Fig. 41.



I am, my dear Sir, yours faithfully,

Robert Stephenson, Esq. William Fairbairn.

The succeeding letter refers to a subject which created great anxiety in my mind, viz. the flexibile nature of the sides of the tubes in immediate connection with the cast-iron framing which supports them. It would be useless to mention the painful reflections, which this imaginary cause of failure produced; it may suffice to observe, that a reconsideration of the experiments on the model tube completely dissipated these fears.

The remainder of the letter refers to the size and dimensions of the pontoons, which were subsequently enlarged according to my suggestion.

MY DEAR SIR, Manchester, November 18th, 1846.

The Colebrook-dale people have been over here, and they want 20*s*. per ton more for the ⊤ iron, in order to enable them to make it of the *best-best* quality. This I objected to; but finally agreed to allow 5*s*. per ton upon it and the angle-iron, on condition that the whole was made of the *very best quality*, which they now agree to do. They will, however, require no increase of price for the angle-iron, provided they are allowed to send in a new tender for the ⊤ iron. To this I objected, and after some cavilling agreed as above.

Now that we are doing away with part of the cast-iron frames for supporting the sides of the tube at the piers, it will be absolutely necessary to attach a series of ½-inch plates at every joint, in order to stiffen that part. I have drawn the plan out in full, and will send you a tracing as soon as Mr. Clarke returns. Evans will be here tomorrow, and I will arrange everything with him, and send the whole of the particulars for your inspection, preparatory to the signing of his contract.

I have been considering about the pontoons, and from the height of the tube, and the great weight of the top, I should doubt whether it would be safe with the small barges. We must bear in mind that the centre of gravity is very high, and unless the vessels on which it is floated be 100 to 120 feet long, we may incur the risk of the whole being so exceedingly crank, as to endanger the safety of the tube. I must go carefully into the matter with Mr. Clarke, and report further on the subject.

I should like to know your movements, and when you purpose going over to France. If you do not leave before Tuesday, I could spend Monday with you in London, and as much of next week as you please.

I am, my dear Sir, faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

The repairs, consequent upon the experiments, on the model tube proceeded almost uninterruptedly from the commencement of these experiments to their close; and it was found, that every succeeding experiment exhibited higher powers of resistance and gave improved results. The question as to the tube failing from lateral pressure had been resumed; and, it was not until the present occasion, that an opportunity was found for putting this important inquiry to the test of experiment. The great elevation of the tubes of the Britannia Bridge, taken in connection with the severe gales to which they would be exposed, created great alarm in the minds of persons well-acquainted with the locality. The damage done by the winds, on more than one occasion, to Telford's chain bridge, was also on record; and taking all the circumstances of position, height, &c. into consideration, a very natural conclusion was, that the safety of the bridge might be endangered.

It must be borne in mind, that the parties who formed this opinion were not acquainted with the rigid nature of the structure upon which we were experimenting; but reasoning from analogy, on more fragile structures, they concluded that the tubes, having such a large surface exposed, would bend to a severe gale of wind, which when blowing in sudden gusts, would give repeated impulses to the tubes, and that thus the lateral oscillations might be multiplied, to such an extent, as to endanger the safety of the bridge. It must be admitted that this reasoning claimed a careful consideration.

The fears thus created were not dispelled, until the experiments, referred to in the following letter, had been made. These experiments were most satisfactory; and the fact of the model tube, when laid on its side, having supported 12 tons with a deflection of only 2½ inches in the middle, was a sufficient guarantee for the lateral strength of the large tubes, and the consequent safety of the bridge*.

Another security, opposed to the strength of the wind, is the enormous weight of the tube itself. The Conway tube (which weighs about 1300 tons), when tested upon temporary piers and left unsupported, was exposed to one of the severest gales of last winter; and the oscillations produced on that occasion, although quite perceptible, did not exceed 1 inch.

During the progress of the experiments and investigation, it had often been stated, that the attempt to erect tubes of 460 feet span across the Menai Straits was chimerical, and that their own weight would prove the principal element of their destruction. To this doctrine I did not subscribe; indeed, from later theoretical investigations, it appears that instead of 460 feet being the limit of the span, 1700 feet are nearer the mark at which a tube would break with its own weight. This span is however greatly beyond practical limits, either as regards the power of obtaining the material, or the cost of construction.

Mr. Hodgkinson's reported experiments on the comparative strengths of square and circular tubes to resist a crushing force, in the shape of pillars, again opened the question as to the admission of the latter into the top of the tubes.

The circular cells are unquestionably superior in strength, but for practical reasons hereafter adduced, it was finally determined to adhere to those of the rectangular form.

My dear Sir, Manchester, December 17th, 1846.

Mr. Clarke would put you in possession of all the facts, connected with our last experiment on the model tube. After subjecting it for 18 hours to a strain of 58 tons on the middle, it was relieved of the load and turned over on its side, when the deflection with its own weight was found to be 0·85 inch. In this position it was loaded with weights of about one ton at a time, till it attained a deflection of 2½ inches with 12 tons. At this stage, it was deemed expedient to discontinue the experiments; and although I am satisfied it would have carried about 21 tons, it was nevertheless desirable not to damage the tube, but to reserve it, with all the parts unimpaired, for the final experiment, which is now in progress.

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* See Experiments in the Appendix.

If we adhere to our original calculations of 50 lbs. on the square foot for the lateral pressure of the wind, we shall find that the model tube, if used as a girder, would have to resist a force of 16,950 lbs. or 7½ tons. Now assuming the limit of fracture to be 20 tons (which I have no doubt the tube would have carried), we shall then have a resistance equal to 40 tons over the whole surface, which is about 5½ times greater than the forces to which it ever can, at any time, be subjected. If we extend this reasoning to one of the Britannia tubes, and again assuming the lateral strength to be 750 tons at a depth of 30 feet in the middle, the resisting powers will then be $750-500$ (half the weight of the tube) $=250$ tons in the middle, or 500 tons equally distributed over its surface. This is irrespective of its own weight; and supposing the tube to be in its vertical position, the lateral power of resistance would then be half its own weight $500+500=1000$ tons, as an antagonist force to the action of the wind.

Again, comparing this force with the actual strength of the wind at 50 lbs. to the square foot, we have an excess of strength in the ratio of 1000 to 280, which I apprehend is more than sufficient to render even the single tube secure. I am aware, that much stress is laid upon the effect of pulsations produced by sudden gusts. All this must be guarded against by looking at the resisting powers of the model, and the very limited deflection produced by heavy weights. My mind is quite released from all fears on this score, and indeed much more so than it was on any former occasion. Viewing altogether the many corroborative facts that have been brought to bear upon this subject, and the important results that have been obtained, it appears to me that we have the most convincing and satisfactory proofs of the accuracy, as well as the importance, of the first and earliest conception of this structure.

It is true that we have it asserted, and from no mean authority, that we are verging upon the utmost limit to which a tubular bridge could be made; but to this doctrine I do not subscribe, as I firmly believe that the present span of the Britannia Bridge may be doubled with our present means; and even then we may be at some distance from the practical limits, which in my opinion can only be circumscribed by the difficulty in procuring proper material, and the enormous cost which a span of 1000 feet would involve. I do not mention this for the purpose of enlarging the difficulties we are contending with, but simply to show, that a work of this kind could be executed, provided it was found necessary and expedient to do so.

Mr. Hodgkinson appears extremely desirous that we should substitute the cylindrical cells in lieu of the rectangular ones. From his last experiments, it is found that cylindrical tubes will bear about 18 tons to the square inch before they become distorted or crushed; whereas the squares will only support from 12 to 14 tons. This gives an excess of 4 tons per square inch in favour of the cylinder.

Now it will be fresh in your recollection, that our first idea of cells was that given in the enclosed sketch, which is traced from the drawings now in your possession. At the time I was convinced of the superiority of this plan, which on mature consideration was abandoned, on account of the difficulty of construction, and the risk of deterioration in those parts that could not be approached. This was the reason for adhering to (what was considered at the time) a weaker form, in order to have an easy approach to every part of the tube, inside and out, for the purpose of repairs, painting, &c. I have repeatedly impressed and enforced these reasons upon Mr. Hodgkinson, but he insists *it can be done*, and to which I do most willingly subscribe, but under conditions similar to those mentioned above. We shall have again to go into the discussion of this part of the subject, and finally decide which of the two is the more eligible to adopt.

In the discussion it must be borne in mind, that the model tube, in its present form, exhibits extraordinary powers of resistance. The cellular top has never yet been seriously injured; and instead of the ratio of 5 to 3, as deduced from the experiments on the smaller tubes, we are likely to have 12 to 10, or probably something approaching equal areas. These increased powers even, according to Mr. Hodgkinson's experiments*, are not obtained from any superiority in the section of the cells experimented upon by him, but from their combined action, and their connection with the sides of the tube, which in every case retains them in their proper position, and proper bearing to resist compression. In my opinion the top of the Britannia tube is in excess; assuming that we give it no further increase, nor alter its form in any way, it will force the bottom to tear through the whole section of solid plate before it can be crushed or distorted in form. Notwithstanding these convictions, I am nevertheless open to the adoption of any other form of top that may be considered more advantageous, and I shall be most willing to return to my original idea of the cylindrical cells, in case they should be found better, and more eligible than those now shown upon the drawings.

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* Or rather according to the experiments on the model tube, since Mr. Hodgkinson's experiments were made upon tubes with single cells, or tubes having no connection with each other, excepting in one case, where two square cells were united.

I am, my dear Sir, always faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

The following letter is simply corroborative of the former, in reference to the practical difficulties, which appeared unavoidable in the construction of the round tubular cells.

MY DEAR SIR, London, December 19th, 1846.

The more I think of the circular cells, the more the practical difficulties increase. If you can see your way to riveting the rectangular cells effectually, I have no question about their being the most eligible, notwithstanding their comparative weakness.

I have desired Mr. Clarke to see you on Monday morning about the arrangement for setting Evans to work, for I am really most uneasy about the time, which is passing away, without some decided progress being made. I fear, unless you see him personally, and get a specific course for proceeding laid down, he will get so far behind, that much embarrassment between us and the Directors and Shareholders will be the consequence.

I have today discussed several points with Mr. Clarke, and he will be prepared to talk to you. The calculations contained in your letter of today coincide almost identically with those which I myself made at Millwall the other day, and appear to leave the question of pressure from the wind in a tolerably satisfactory position. I would only now call your particular attention to the riveting, upon which your judgment must be our principal guide.

Yours faithfully,

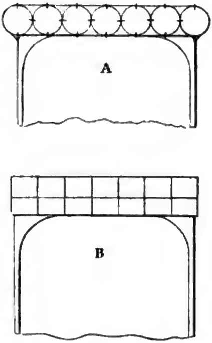
William Fairbairn, Esq., Engineer, ROBERT STEPHENSON.
Manchester.

It will be seen, that every succeeding experiment sufficiently demonstrated the great powers of resistance presented by the square cells. The following letter still further exemplifies the inconvenience which would attend the introduction of the circular ones. These facts and statements had their due weight with Mr. Stephenson, and hence the rectangular form of the cells was finally adopted, in preference to those of the circular kind.

After giving the results of the experiments on the model tube, I proceed to calculate the strength of the large tube, from the data derived from these experiments. The result of this calculation was highly satisfactory, and inspired confidence in the strength of this form of tube. The details of these experiments are given at the close of the letter.

MY DEAR SIR, Manchester, December 20th, 1846.

Fig. 42.



Since my last letter on the subject of the tubes, I have come to the conclusion, that we cannot practically alter the form of the top, without incurring a risk of evils greater than the benefits derivable from the cylindrical cells. I am quite aware of the advantages belonging to the circular cells, but the impediments, which all along have deterred their application, is the difficulty which presents itself in rendering all the parts of the tube accessible. You will easily recall to recollection the power, with which you dwelt upon this circumstance only two months ago; and which caused me (in deference to your superior judgment,) to give up the strongest form, as at A, for the one now submitted, as at B. There cannot be a doubt as to the superior resisting powers of the former, compared with the latter; but in order to attain the maximum strength of the cylindrical cells, we must unite them by a top and bottom plate, and also rivet them to each other throughout the whole length of the tube. This must be done, in order to attain the greatest possible resistance, and to ensure their acting in combination, to give support to the whole, and to each other. They would require this unity of action, under every condition of the weight of the tube and its load. It is true, Mr. Hodgkinson has shown what was already known to both; namely, the superior strength of the cylindrical cell; and further, he has also shown wherein that strength consists; but he has not shown how it can be applied in this case, without incurring evils, probably greater than those they are intended to cure. If I am right in the arrangements proposed on this and former occasions, it is evident that the intersections between the connexion of the different tubes cannot be got at, whether for the purpose of cleansing, painting or repairs. This appears to me, as it did to you in the first instance of their suggestion, an insurmountable barrier to their introduction, and an equally forcible argument in favour of the rectangular cells, as now suggested, and partially approved of. I am however still open to conviction on this matter, either from yourself, Mr. Hodgkinson, or Mr. Clarke, provided any better form of cell can be found, so as to meet all the requirements necessary for ensuring the *strength and durability* of the tube. I am the last man to oppose its introduction.

For the present, however, I see none so well calculated for these objects as the rectangular form, and I believe the experiments at Millwall fully confirm the security, as well as the durability and great practical convenience, of this principle of construction.

The experiments, made at Millwall on the model tube, are probably the most valuable and most conclusive of the whole series; and provided we are assured, that the tubes for the Britannia and Conway Bridges follow the same law, as regards their powers of resistance to a tensile strain, the inference would be, in reasoning from the one to the other, that the large tubes, as now laid down, are amply sufficient to resist not only their own weights, and the heaviest loads that can be placed upon them, but also the external force of the wind under whatever conditions it may act upon them.

If we revert to the last experiment but one (October 16th), it will be found, that the plates which composed the bottom of the tube were of inferior quality, and consequently they gave way by tearing asunder with a weight of 66 tons*. Since that time, the bottom has been increased by a superior quality of new plates, raising the sectional area of that part from 17·8 to 22·4 inches. These plates have however only been extended to a distance of 21 feet on each side of the centre; and, consequently, from that point to the end of the tube, the bottom has undergone no change, but continued with the original plates of 8·8 inches area.

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* See experiments in the Appendix.

It is to be regretted that one of them had not extended a few feet further, as it would have equalized the bottom, and considerably increased the bearing powers of the tube; had this been done, instead of breaking with 69 tons, it is more than probable that 80 tons would have been the nearest approach to its maximum strength. But be this as it may, I should consider myself safe, in estimating the breaking weight of a well-proportioned tube of this description at 75 tons; and taking into consideration the same strain already sustained at this point of the tube, and its tortured condition in other respects, I am clearly of opinion that 75 tons is rather within than beyond its bearing powers.

Assuming however 75 tons to be the ultimate strength, we should then have for the Britannia tube (as the cubes and squares of the strengths and weights respectively), a tube weighing 1200 tons, and carrying 2570 tons in the middle, *inclusive of its own weight**.

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* Here we have, from equations (5.) and (7.), p. 104,

$$W_{,}=\frac{l_{,}^{2}}{l^{2}}\,.\,W \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

and

$$w_{,}=\frac{l_{,}^{3}}{l^{3}}\,.\,w \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

From equation (2.), we have

$$\frac{l_{,}}{l}=\left(\frac{w_{,}}{w}\right)^{\frac{1}{3}},$$

substituting in equation (1.), we find

$$W_{,}=\left(\frac{w_{,}}{w}\right)^{\frac{2}{3}}.\,W \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

$$\therefore \quad \frac{W_{,}^{3}}{W^{3}}=\frac{w_{,}^{2}}{w^{2}},$$

which is the formula enunciated in the letter.

Let $w=6$, $W=75$, and $w_{,}=1200$, then we have by equation (3.),

$$W_{,}=\left\{\frac{1200}{6}\right\}^{\frac{2}{3}}\times 75=2570 \text{ tons nearly.}$$

Adopting the notation of the formulæ given in page 104, equation (3.) becomes

$$L_{,}+w_{,}=\left(\frac{w_{,}}{w}\right)^{\frac{2}{3}}.\,(L+w),$$

$$\therefore \quad L_{,}=\left(\frac{w_{,}}{w}\right)^{\frac{2}{3}}.\,(L+w)-w_{,} \quad . \quad . \quad . \quad (4.)$$

Another expression for the breaking load is given in equation (8.), p. 105.

In the experiment of Wednesday last, there is sufficient evidence to show, that the weakest portion of the tube was at the point where the new plates terminated; and that the cause of fracture did not arise *from the weakness of the sides* or *want of rigidity in the cellular top,* but from defective proportions in the material in that part calculated to resist tension.

These views are sufficiently apparent from the fact, that the sides and cellular top (which from the commencement have never been varied in the thickness of the plates) have sustained no injury, excepting from the weakness of the bottom, which in every case of rupture injured both.

It will further be observed, that the sides and top were of equal strength and thickness throughout; consequently they had to sustain a much greater strain in the middle, than at the part where the fracture took place, clearly showing that the sides and top are the strongest parts of the tube. Another circumstance corroborative of this fact is, that the tube gave way with a deflection only of 3·81 inches; whereas in some of the former experiments, the deflection attained was as much as 5½ inches before fracture occurred. Now, if the sides and top had been the weaker portion of the structure, they must of necessity have been the first to give way; but such was not the case in any one of the series; and hence will follow the inference that the relative proportions of the top, bottom and sides, as given in the last experiment (if properly proportioned), would be the nearest approach to the section of greatest strength.

The details of the last experiment are below, which you will oblige me by giving to Mr. Clarke.

Meanwhile, I am, my dear Sir, faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

Experiments, December 14*th,* 1846.

No. of experiments.	Weight in pounds.	Deflection in inches.	Remarks.
1	2,240	0·00	
2	11,568	1·70	
3	20,169	1·31	
4	32,133	1·91	Weight left suspended from the 14th to Wednesday the 23rd, when the deflection had increased from 3·17 to 3·22. Permanent set 0·05 inch.
5	54,185	2·38	
6	79,548	2·77	
7	105,883	2·96	
8	135,255	3·17	

Experiments (continued), December 23rd.

No. of experiments.	Weight in pounds.	Deflection in inches.	Remarks.
9	139,567	3·38	Broke at a distance of 21 feet 4 inches from the shackle by tearing across the bottom. Area of bottom at the fracture 8·8 inches. The weight 154,452 was suspended for about three minutes before the tube gave way.
10	144,352	3·48	
11	147,684	3·70	
12	151,772	3·81	
13	154,452		

The following communication, which refers almost exclusively to the nature of the material and the manufacturing of the plates, shows the preparations that were making for the construction of the tubes.

MY DEAR SIR, Manchester, December 26th, 1846.

On separating from you at the Wolverhampton Station on Monday last, I proceeded direct to Thorneycroft and Walter Williams. In the works of the former, I found all the E plates for the bottom of the Conway tubes rolled, and a good job.

At Walter Williams', 300 tons of plates (for the sides of the Conway) were ready for delivery, and also well-sheared, and in other respects sound and good. The Bradley Company (Foster and Co.) rolled some angle-iron, but have as yet done nothing at the plates; but Burrows and Co. can deliver 40 or 50 tons of angle-iron in the first week of January, and Thorneycroft will have some of the ⊤ iron ready about the same time. The Colebrook-dale Company have rolled about 10 tons of plates, but they are badly sheared, and I am now writing to them to devote more attention to that part of the manufacture. I am also urging despatch with the Butterley Company, and hope to have abundance of material for the construction, as soon as they are ready to receive it.

As respects the makers of the tube, I have seen them all this week, except Horton, and I am now able to report as follows:—

1st. That Evans states he will be ready to commence with the bottom of the tube by the middle of next month. His workshops and steam-engines are nearly completed; and the platform for supporting the tube, and on which it is to be constructed, is in progress, and will be finished about the 15th of January. By the bye I will send you a tracing of this platform and the pontoons in a few days.

2nd. Messrs. Mare and Ditchburn will commence punching on the 13th or 14th of the ensuing month, and after their first delivery, (which will be about the beginning of May), they will deliver about 100 tons a week. Should these terms be correct, we must not lose much time with the platforms at the Straits.

3rd. Garforths say that they will be ready for thin plates in about three weeks, but from what I can see of their operations, I fear it will be double that time. I shall, however, attend to them.

Lastly. Horton is in the same state.

Such is the present rate of progress, and such are the prospects we have before us; and, provided you consider it necessary, I will draw up a concise report to the above effect, and lay it before the Directors at their next meeting. Or, in case you should be present, your verbal statement may probably be sufficient.

I shall be in Ireland for a few days the week after next, till then I am at home, and will be glad to hear from you. Meanwhile,

I am, my dear Sir, always faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

The increasing expenses consequent upon the experiments still in progress both at Millwall and Manchester, gave considerable uneasiness to the Directors; and moreover a joint contract, which had been entered into by my son, as representative of the firm of Messrs. Fairbairn and Sons, Millwall, with Messrs. Ditchburn and Mare of Blackwall, for constructing the greater part of the tubes for the Britannia Bridge, was looked upon with suspicion by the Board. Although interested as a partner, I had not personally interfered in the matter, and I was even unacquainted with the terms of agreement between the two firms; but when the feelings which were entertained by the Directors were made known to me, and as it appeared difficult for me, in consequence of the partnership, to maintain a perfectly independent position, I urged a transfer of the whole contract into the hands of Messrs. Ditchburn and Mare: this transfer was afterwards satisfactorily arranged by my son and Mr. Mare, and approved of by the Company.

In the following letter, dated January 15th, 1847, some explanations are entered into relative to the nature of those expenses, showing that the money expended, instead of being a loss, would ultimately prove a source of economy, infinitely more important than any saving which could be effected by a limitation of the experiments. The succeeding parts of the letter treat of the greatest extent of span to which a tubular bridge could be carried, before it would break with its own weight.

My dear Sir, Manchester, January 15th, 1847.

In the great undertaking in which we are engaged, I have been actuated from the commencement, with a desire to attain the objects in view in a manner the most efficient, and at the least possible expense to the Company.

If the "cutting remarks" made by the Finance Committee and the Board of Directors apply personally to myself, and to the validity of the charges I have made for the experiments, I should have no alternative but to demand a searching inquiry into the whole of the accounts, and an entire cessation of all further experimenting on their account. I should further insist upon being entirely relieved from any description of contract, that I may leave myself perfectly free from all suspicion as to my position with the Company.

On the other hand, should the observations made by the Board apply to the expenses generally as incurred by the experiments, I should then think it desirable to put a stop to the whole, and to give Mr. Hodgkinson notice to that effect. I am far from depreciating the value of Mr. Hodgkinson's experiments; I have no doubt they are valuable; but they are so closely kept, that I am quite unable to form any opinion, how far they bear upon the question of the tubular bridge. But be that as it may, I shall obtain great relief as soon as these experiments are brought to a close.

On the probable time when Mr. Hodgkinson will finish, I am unable to obtain from him anything definite. You must therefore write to him, and let me know what is to be done. If this is not done, it will be impossible for me to keep six or seven men (exclusive of boiler-makers, &c.) constantly employed, unless their time is charged to some person or other.

I have written in reply to a letter from Mr. King on this subject, and I did not hesitate in stating, that the Directors were little aware of the trouble and expense incurred in the prosecution of these experiments. To me, as a matter of business, they are no source of profit, but quite the reverse; and provided the Directors take a just view of the question, they will find that these very experiments, so much condemned for their expense, have saved two or three hundred times the amount. I do think, if we are to be charged with an apparently expensive outlay in this inquiry, they should also take into account the benefits they will ultimately derive from it, in the saving of chains and other advantages, which these experiments have developed. On the whole I am quite willing to stop, as I think the experiments have nearly gone as far as may be considered necessary; and what more is to be done I will do at my own expense, and again repair the Millwall tube.

Mr. Clarke has just arrived from Conway, where he left Evans making good and satisfactory progress. I am going over to stop some days with him, as soon as the plates arrive, and to give him a fair start. Mr. Clarke promises to write to you and tell you all about it.

I have been calculating the theoretical extent to which a tube of the description we are making could be carried, before it would break with its own weight; and I find from data deduced from the model tube, that the limit is about twenty-four times the dimensions, or a span equal to 1800 feet nearly. I do not mean to say that we could find material, or that it is practical to build such a tube; but I am persuaded, in case we are successful with the Britannia and Conway, that a bridge 1000 feet span might be made*.

I however leave these gigantic structures to our successors, being con-

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* Here making $L_{\prime}=0$ in equation (8.), p. 105, we have

$$\left(\frac{l_{\prime}}{l}\right)^2 \left\{ L - \frac{l_{\prime}-l}{l} \cdot w \right\} = 0 ;$$

$$\therefore \quad l_{\prime} = \frac{l}{w} (L + w) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

Taking $l=75$, $L=56=56{\cdot}3$, and $w=\frac{5{\cdot}15}{2}$, according to the experimental data of p. 109, we have

$$l_{\prime} = \frac{75}{2{\cdot}575} (56{\cdot}3 + 2{\cdot}575) = 1710 \text{ feet},$$

which is the length at which the tube would break with its own weight.

scious, so far as respects our present undertaking, that we are very considerably within the limits of destruction.

Shall we have the pleasure of your company, if only for one day?

I am, my dear Sir, faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

The following letter from Mr. Stephenson has reference, almost entirely, to the observations made in Committee about the expensive nature, &c. of the experiments.

MY DEAR SIR, No date.

The remarks made by the Finance Committee were intended to apply to me just as much as to any one else, and in looking into the accounts, I did not feel prepared to state that all the charges were reasonable, because I had never seen them; indeed I did not know that they had been delivered.

You are quite mistaken when you talk of suspicion; nothing of the kind exists; therefore don't make yourself uneasy by imagining that there is. The real cause of the remarks arose from our having stated in the first instance, that the experiments we proposed to make would cost under £3000, whereas now they appear likely to exceed £6000.

I have received Mr. Clarke's report of the progress made by Evans, which I think is satisfactory; but you must look him up about the time he commences riveting, as much will depend upon the system he adopts, and I hope this will have your especial attention and judgment.

If I could have left London this evening, I would have had much pleasure in spending tomorrow with you and my father, who I hope is now with you. I shall most probably write to Mr. Hodgkinson tomorrow to put an end to any new experiments, for I am satisfied we cannot now give much more essentially useful information.

Yours sincerely,

William Fairbairn, Esq., Manchester. ROBERT STEPHENSON.

The ultimate strength of the tubes had always formed a subject of discussion. Mr. Hodgkinson entertained fears relative to the safety of the bridge, and great pains were taken, from time to time, to reconcile him to the form of cells that had been decided upon for both bridges. Finding new elements of resistance in the angle-iron which connects the square cells, he gradually but reluctantly came over to our views, and no longer objected to this construction. It was a source of considerable satisfaction, when that gentleman began to adopt more favourable views of the structure.

The mode of supporting the extreme ends of the tubes, in order to admit of their expansion and contraction, had been often discussed; and the arrangement of bed-plates and rollers below, with bullets rolling in troughs above, was finally determined upon and carried into execution, as shown in the drawings.

My dear Sir, Manchester, January 24th, 1847.

I am in receipt of Mr. Clarke's note of yesterday, and am glad to find that a model is preparing of the rollers and other apparatus, connected with the support and expansive action of the tubes. I should be glad to spend a few days with you, to consult and finally determine upon this subject, as also upon the raising apparatus, as soon as you are ready for me.

In the interval, I hope to spend a few days at Conway with Mr. Ross and Mr. Evans, and to have the satisfaction of reporting a commencement of the construction of the tubes. I shall also spend one or more days with Mr. Foster at the Straits, and endeavour to expedite and consolidate our operations in every department.

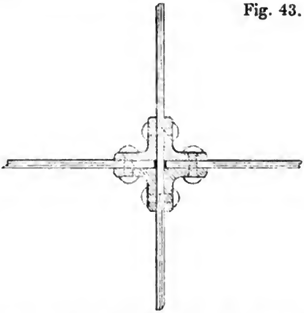
I am also happy to inform you, that my old friend Mr. Hodgkinson is gradually coming round to our views with regard to the stability of the cellular top. He brought up a whole string of calculations for my perusal and consideration on Friday. My limited knowledge of mathematics would not admit of me following him into the more abstruse part of his computation; but I discovered sufficient to show, that he was reconciled to the rectangular shape, as he found the angle-iron in each corner gave great resistance to a crushing force, and in many respects rendered it an approximate, if not nearly equal to the cylindrical forms. The fact is, he appears to have overlooked the quadruple strength and action of the angle-iron, as per annexed sketch, which forms almost the principal element of the strength of the cell.

Altogether I however think, that Mr. Hodgkinson will come to the conclusion, that we run no risk in adopting the present system of the cellular top.

In addition to his altered views, I find that his computations as to the strength of the Conway tube are not widely different from my own, namely, 8 tons on the square inch, which gives a resisting power equal to nearly 1000 tons, exclusive of its own weight; this is however not fully determined, but it will be so in a few days, when, I have no doubt, the whole of the calculations will be laid before you. I am myself working at it by a less abstruse and more simple process, and I shall be very glad, when we compare notes, to find them agree, or at least approximate closely to each other.

I shall be most happy to render whatever assistance I can, in reference to the mode of erecting and raising the tubes. There are several points which appear to me to require consideration, and as soon as you are sufficiently advanced with the model*, you may command,

Fig. 43.



My dear Sir, yours faithfully,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

Mr. Hodgkinson's computations, relative to the resisting powers of the Conway tube, *apparently* differed considerably from mine. The following letter contains an attempt to reconcile these differences, and to give confidence, not only in the accuracy of the experiments, but also in the calculations derived from them. Professor Airy, the Astronomer Royal, in a letter addressed to Mr. Stephenson, had given it as his opinion, that the strength of wrought-iron tubes varies as the square of the length, and the weight as the cube of the length. Taking these data, and assuming a large tube (such as that across the Menai Straits,) to be an exact proportional of the tube experimented upon at Millwall, I proceed to calculate the breaking weight of this tube, and find that the result exactly coincides with that obtained by the formula, which I had before used.

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* The model here referred to was not made, but drawings of the whole of the machinery connected with the raising apparatus, bed plates, rollers, &c., were prepared at Manchester, and a model one twenty-fourth of the full size was constructed from these drawings.

My dear Sir, Manchester, February 2nd, 1847.

I am now most anxious to have all the proportions of the tube finally settled, as I find we have some difficulty in procuring the plates from the makers in time. I have ordered considerable quantities from each maker, but, with the exception of Thorneycroft, I do not find one of them prepared to undertake the rolling of the large plates; indeed so difficult do they find them in both rolling and shearing, with the machinery they possess, that I much fear their operations will be slower and more tardy than we expected.

I however expect to find them better prepared in a week or ten days; but at present, none of them have been able to deliver a single plate of the large size (for the bottom) except Thorneycroft; and those have gone to Evans. I expect he will send a quantity to London this week; but he cannot possibly do them all, and every means possible is adopted to urge the others forward. The greater portion of the plates for all the tubes are ordered, except the variable plates for the sides of the Britannia tubes. I have determined, with your approval, to adopt the following form and dimensions, the curvature of the top being nearly a parabola as above.

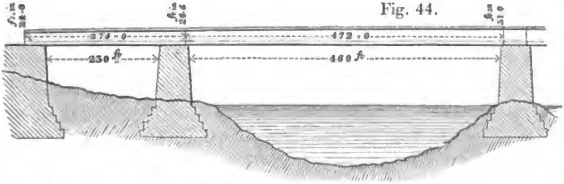

Fig. 44.

Our decision on these points would enable us to order the whole of the variable plates, and complete the sides. The top, as a matter of course, will stand over till we meet in London, which I hope will be on the 10th instant, when I propose to be in town, and to stop with you till everything is settled.

You will pardon me for troubling you with so many letters, which you have probably little time to read, and less time to answer. It is, however, necessary that I should inform you, that I have had another interview and discussion with Mr. Hodgkinson on the strength and resisting powers of the Conway tube; and although we are not so far separated in our ideas as to form and strength as we were before, we are still not so closely allied in our computations, as to ensure perfect agreement. It is true, I have not gone into such a laborious and abstruse calculation as Mr. Hodgkinson, nor is it in my power to do so. I am, however, sufficiently acquainted with practical mathematics to compute the resisting power of these tubes; and resting the basis of my calculations entirely upon the experimental facts as deduced from the model tube at Millwall, I am strongly inclined to think they are not far from the truth. I will however briefly compare them for consideration, and at some leisure hour you can run them over, and give me your opinion.

It appears, from data furnished by Mr. Hodgkinson, that the Conway tube, which is 400 feet span, and 27 feet deep, with plates of the thickness as given in the drawings, has a strength, including its own weight (the plates being considered continuous, and without joints), of 12 tons on the square inch $=1485$ tons; and the deflection with the same strain (12 tons per square inch) is $=13{\cdot}97$ inches. Now, Mr. Hodgkinson allows one-third for the joints and rivets, which reduces its bearing powers to 990 tons; and taking from this again one-half the weight of the tube, it leaves for the actual strength $990-450=540$ tons, or 1080, say 1100 tons, equally distributed over the surface of the tube. Assuming the above 1100 tons to be the maximum strength, we shall next have to consider the strain to which, under the most unfavourable circumstances, it may be subject. Supposing therefore that the rails and roadway be equal to 100 tons, a train with locomotives and tenders, &c. 360 tons, we have a total load of 460 tons; leaving for resistance to the vibrating action of the strain 640 tons. This, although perfectly secure, is rather too fine for actual practice. The above constitutes the powers of the Conway tube, as arrived at by Mr. Hodgkinson. They are however taken at a strain of 12 tons to the square inch; but I believe this is not the breaking weight, as I think that Mr. Hodgkinson, in his recent experiments on single and uncovered tubes, gave to the crushing force 14, and in come cases 16 tons to the square inch. Taking it however at 14 tons, it is certainly not too much to infer, that, in the combined cellular top, 16 tons would be a fair pressure for the maximum of strength. Should this be the fact, then Mr. Hodgkinson's theory and calculations come up to my own, or nearly so.

I will now take my own method of computation and that of Professor Airy, and see how far they agree with that of Mr. Hodgkinson. In the discussion of this question, I will take the weight of the model tube at 6 tons, the distance between the supports at 75 feet, the depth 4 feet 6 inches, and the breaking weight of the tube at 60 tons.

Now assuming, according to Airy, that the strength will be as the squares, and the weight (of any tube being an exact proportional) as the cubes; and taking the Conway tube at 400 feet span, or 5·33 times the size of the model in every respect, we have $5{\cdot}33^2 \times 60 = 1704$ tons as the strength, including the weight of the tube, and for the weight we shall have $5{\cdot}33^3 \times 6 = 906$ tons. Hence $1704 - 453 = 1251$ tons, the actual strength of the tube exclusive of its own weight*.

Now comparing this with Mr. Hodgkinson's calculations, we shall have more than double the strength, or as 550 to 1251, which is equivalent to an equal distribution of 2502 tons over the surface of the platform of the tube, more than five times the weight it will ever be called upon to sustain.

In this stage of the inquiry, I must, however, do Mr. Hodgkinson the justice to say, that we do not differ so widely as might at first appearance be supposed. In the first place, Mr. Hodgkinson takes the strain per square inch at 12 tons, whereas, according to his own showing, it will sustain a pressure of 14 tons. This will raise its bearing powers to upwards of 1730 tons; and then he allows one-third for the joints, while

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* Here, by equation (8.), p. 105, $l=75$, $l=400$, $w=\frac{6}{2}=3$, $L=60-3=57$;

$$\therefore \quad L_{,}=\left(\frac{l_{,}}{l}\right)^2 \left\{L-\frac{l_{,}-l}{l} \cdot w\right\}$$

$$=\left(\frac{400}{75}\right)^2 \left\{57-\frac{400-75}{75}\times 3\right\}=1250 \text{ tons.}$$

in fact, by our new principle of riveting, we shall only lose about *one-seventh.*

Taking therefore Mr. Hodgkinson's computation at the maximum, or 1730 tons, minus half the weight of the tube, and one-seventh for the jointed plates, we have for the actual strength in the middle 1009, which is much nearer the mark, and it gives a larger margin for security under every condition and circumstance in which the Conway tubes can be placed.

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Faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

P.S. In consequence of Mr. Clarke's arrival, and hearing that you intend to visit Bangor about the 12th or 13th, I have sent J. M'Laren, the person I have appointed as the inspector of the tubes to Mr. Evans at Conway, to arrange all the tools, and start the punching of the plates and angle-iron. He will write to me from Conway tomorrow, and if necessary, I will run over on Saturday, and return direct to London, in order to join you at the meeting of the Board on Wednesday the 10th. If I do not go to Conway till next week, I will go to the Butterley Works on Monday on my way to town.

After the commencement of the experiments on the model tube, Mr. Clarke was a constant attendant. He recorded every transaction, and reported regularly to Mr. Stephenson everything that was done. In all respects he became a most intelligent and valuable assistant; his mathematical attainments, and the aptitude with which he took up almost every practical detail, soon made him well-acquainted with the subject; moreover, having his time at command, and the whole energies of his mind concentrated on one object, he attained a degree of expertness highly creditable to the talents of a gentleman whose previous knowledge of practical mechanics was not extensive.

MY DEAR SIR, 24 Great George Street, February 5th, 1847.

I have to apologize for not returning to Canal Street, but Mr. Hodgkinson detained me till the last moment, going through an enormous mass of x's and y's; and, even then, I was obliged to come away without any copy of his investigation, on account of some little *clerical* errors we detected. He however promises to send me a copy, and I will condense the matter and report to you on the results of it all. I told him to discontinue the experiments at once, until I inquired of Mr. Stephenson as to his finishing the 2nd or 3rd experiments he has begun. He wants another month. I find Mr. Stephenson still absent, but I believe he is expected tomorrow. I understand, moreover, he intends to delay his visit to Wales. It will be well to remove the things from the yard where the experiments were made, to be ready for giving it up.

Believe me, dear Sir, yours very faithfully,

William Fairbairn, Esq. E. CLARKE.

A series of experiments, made about this time by Mr. Hodgkinson on the resisting powers of tubes to compression, gave favourable impressions as to the application of cast-iron on the top of the tubes for the Britannia and Conway Bridges. This application I strongly opposed, for reasons assigned in the following letter.

For some time previous to this communication, the properties, strengths and proportions of both bridges had been carefully calculated and determined upon. The top with two rows of cells presented immense powers of resistance; but considerable practical difficulties had to be encountered in the construction. At the same time, the increased strength exhibited in the single cells, during the successive and repeated experiments upon the model tube, was of such a character as to lead to the adoption of a similar form in the construction of the large tube. The application of these facts was readily determined upon on account of the difficulties already mentioned; and the single row of cells was unanimously approved of, as equally effective, and infinitely more simple of construction. At the same time the strength of the bottom plates was increased, until the ratio of the bottom section to the top became as 500 to 565, or as 1 to 1·13.

MY DEAR SIR, Manchester, February 27th, 1847.

Having come to a decision, I am exceedingly unwilling to make any change; and nothing should induce me to do so but a conviction that it is for the better. You will be in possession of a letter from Mr. Hodgkinson, detailing the results of his last experiments, wherein much stress is laid upon the use of cast, in combination with wrought, iron to resist compression on the top of the Conway and Britannia tubes. Now without disputing the accuracy of these experiments, or the conclusions at which Mr. Hodgkinson has arrived, I would submit, that, so far as I am a judge, they are not such as would induce me to substitute the one material for the other. On the contrary, I am more convinced than ever, that the cellular top composed of rolled plate-iron is infinitely superior, and better calculated to resist compression, than any form of cast-iron that can be introduced; that is, the weight being the same, and the security of the structure being a desideratum in every case. I have come to this conclusion, not from any partiality to my own opinions, but from a conviction that cast-iron is neither applicable nor so well calculated for the construction of large tubes as wrought-iron, and that for the following reasons:—

1st. Because cast-iron is inferior to wrought-iron in particular positions, as already shown by Mr. Hodgkinson, in resisting a compressive force up to 14 tons upon the square inch.

2ndly. As a compressive strain of 14 tons upon the square inch cannot at any time be laid upon the top of the Conway or Britannia tubes, the use of wrought-iron must be preferable to that of cast-iron.

Lastly. Cast-iron, from its brittleness and crystalline texture, is not so safe, nor yet so well adapted for the construction of large tubes, practically considered; nor is it advisable theoretically, if wrought-iron be superior in its powers of resistance (up to 14 tons) to a crushing force.

Besides, I cannot consider the experiments, recently made by Mr. Hodgkinson, as at all applicable either to the form or the construction of the tubes. They are too much of an abstract character; and although they appear to corroborate certain isolated facts, they are still far from bearing direct upon the question, as to the form of material necessary to be used for the top of the tubes. You are aware as well as myself, that the whole of Mr. Hodgkinson's experiments have been conducted differently to those which I have made. In his, the tubes have been invariably supported in the middle with a frame of cast-iron, and a cross cushion of wood which formed the fulcrum, over which the fracture was made. Now it is evident, that this is not the position in which the tubes will be placed: they have no frame in the middle, nor yet any cushion on the top to keep them steady; they are, in fact, unsupported from one tower to the other when permanently fixed; they will be in the same position as the model tube at Millwall, with an opening through, and with no other support, but what is derived from its own stiffness and the tenacity of its parts. By taking therefore the model tube (and I always come back to it as our guide), I am satisfied we are much safer, and much more certain of due and perfect proportion, than if we calculated from experiments abstractedly taken from tubes disproportioned in all their parts, either as regards the thickness of the material, or the length or depth of the tubes.

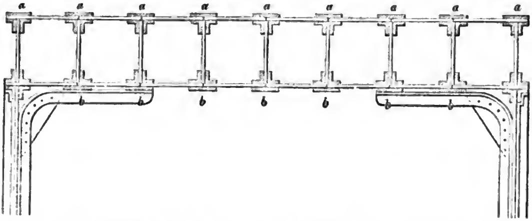
I admit, it may be possible for mathematicians to deduce numerous important facts from such experiments; but with every deference to these useful and distinguished men, I have not yet heard of anything to cause a departure from the Millwall tube.

If we take that tube, with its numerous and important facts, all bearing (in due proportion) upon the full-sized structure, and compare it with the abstract reasoning of the most profound mathematician, I am persuaded we shall be at a loss, as to what should be done in the construction and form to be adopted in the large tubes. I mention this, not from any desire to lessen the value of Mr. Hodgkinson's experiments, or their importance in a mathematical point of view, but to lay before you the reasons why I adhere with such tenacity to opinions, which, in every instance, have been confirmed by their superior importance. I have talked this matter over with Mr. Clarke, and I think, if I understood him correctly, that we cannot depart from the principle of the cellular top, excepting in certain modifications to which we are both agreed, and on which we wish to have your opinion and advice.

The modifications to which I allude are as follows;—viz. to construct the top of much thicker plates, $\frac{3}{4}$ instead of $\frac{1}{2}$ inch, and to form only one series of cells, containing as nearly as possible the same sectional area, and presenting the same form of resistance to the "puckering" and crushing tendency of the material. If this were accomplished, (and I see nothing to prevent it), the construction would be much more simple; and, what is of still greater value, increased facilities would be offered for the reconstruction of the Britannia tubes at the Straits. This is a matter of great importance, as the chief difficulty I have had to encounter is the riveting of the vertical joints of the tubular series in a space, not exceeding 18 inches high, and 18 inches wide. If we adhere to the double tier of cells, I fear I must have the whole of the top plates loose, in order to effect sound and efficient riveting of the internal joints. Now, by the adoption of only one depth of cells and stronger plates, it will bring us to the same construction as the model at Millwall, which, in every instance, has exhibited a degree of resistance to compression considerably beyond that of tension on the lower side of the tube.

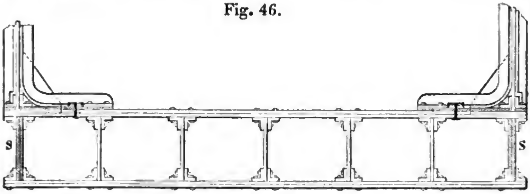
I have already stated my reluctance to change, and I would not on this occasion have suggested any alteration, as I am satisfied we have now a good top, and I will abide by it, if you think proper; but my desire is, to lessen the expense of construction, and add what we save in weight in the top to the bottom of the tube; that is, to proportion the area of the top and bottom of the tube respectively in the ratio of 5 to 4. This would bring the form, as well as the sections, a near proportional of the Millwall tube, which on every occasion, and at every test, has given repeated instances of the superior strength of the cellular top. I have carefully considered this matter over with Mr. Clarke, who is even more anxious for the change than myself; and after mature deliberation, I have to propose that a cellular top, of the following proportions and dimensions, be substituted for that with the double tier of cells.

Fig. 45.



The above cells to be 1 foot 8¼ inches square; all the plates to be ¾-inch thick, with stronger angle-iron, and ½-inch strips above the top, and bottom, as shown at *a*, *a*, *a*, and *b*, *b*, *b*, &c. The above construction would give much greater facilities for riveting the joints of the vertical plates; and, from the increased thickness of the plates, we should not only diminish more than half the number of cells, but I apprehend, from a few additional plates introduced at the bottom, we should have a stronger tube. The increase I should propose for the bottom would be simply as under.

Fig. 46.



Double plates for the bottom as before. The two outside vertical plates at SS *double*, and an additional line of plates over the two outside cells at II. With these curtailments and additions, the areas would stand as follows :—

Bottom	500	square	inches.
Sides	259	,,	,,
Top	565	,,	,,

If you agree to these proposals, let me hear from you at your early convenience ; or in case you deem it proper, I should be glad to see you here for a single day, in order to come to a final decision. We must now order the plates without a moment's delay, and the sooner it is done the better.

I am, my dear Sir, yours faithfully,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

The succeeding letter contains Mr. Stephenson's approval of the proposed change in the cellular top, accompanied with an expression of confidence in the use of cast-iron in certain particular cases.

MY DEAR SIR, London, March 3rd, 1847.

My letter to Mr. Clarke would convey my concurrence in the proposed change in the arrangement of the top and bottom of the tube, and you would also observe, that I proposed to increase the dimensions of the top cells, precisely as you suggested in your letter of yesterday. I attach some importance to the use of cast-iron ; and in several bridges now in the course of construction, the top side of the tube is stiffened by this material, but this need not interfere with our present determinations in reference to the Conway or Britannia. I hope, therefore, that the orders will shortly be all in the hands of the different manufacturers. I find I have an appointment at Lynn on the 9th, from which place I intend proceeding to Manchester, when I hope we can make an excursion amongst the iron-masters. Trusting that this will be convenient to you,

I am, yours faithfully,

William Fairbairn, Esq., Manchester. Robert Stephenson.

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24 Great George Street, Westminster,
March 31st, 1847.

My dear Sir,

I have sent Clarke down to Conway to arrange with Evans, for proceeding with the masonry in the upper part of the tubes, where it is likely to be interfered with by the cast-iron girders. I am glad to hear, that you have given Blair positive instructions to see all off that is wanted by the builders.

I am leaving town on Thursday evening for a week: on my return, we must arrange to meet the Board on the subject of the lifting apparatus. I have no doubt it is all right as to Evans's certificate.

I hope you will arrange to have nearly, if not all, the bottom riveting done by the machine, it is so infinitely superior to anything that can be done by hand.

Yours faithfully,

William Fairbairn, Esq., Manchester. Robert Stephenson.

Owing to the enormous strain exerted upon the cast-iron frame-work and cross beams, very great attention was required in their construction. See the description, hereafter given, of the method of raising the Conway tube.

My dear Sir, Manchester, March 24th, 1847.

I have now completed, or nearly completed, the whole of the drawings for the frame-work, girders, &c. for lifting the tubes. The arrangement of the hydraulic apparatus, chains, &c. is also complete; and as soon as we have copied the drawing, the whole shall be laid before you. I am now well satisfied as to the security of the ends of the tubes, where the chains are to be attached, as also the large girders, and all the roller platforms, which are now secure and in a most satisfactory position.

Mr. Clarke took with him the tracings for the hydraulic presses, and drawings of those portions of the towers which require the attention of Mr. Thompson, and where it will be necessary to have recesses left for riveting and uniting the middle parts of the tubes. All these points are now clearly developed, and I trust in a way equally satisfactory to you as to myself.

I have appropriated the greater portion of this day in writing to all the Contractors and Inspectors *urging despatch*, and again reminding them of the great necessity which exists, for having the work *sound and perfect in every respect*. I have laid a good deal of stress upon them in this respect; but as soon as they are fairly started, I shall convince myself of their progress by a careful and rigid inspection. As soon as Mr. Clarke returns, we shall finish all the drawings, and have the lists and specifications prepared to lay before the Directors, in order to have the tenders and contracts completed for the raising apparatus, girders, frames, &c.

I am, my dear Sir, faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

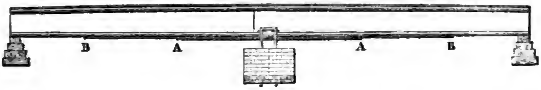
In all the previous experiments on the model tube, the rigidity of the cellular top resisted every attempt to crush it; the fracture in every experiment having taken place at the bottom plates. It was therefore absolutely necessary that the experiments should be repeated, in order to crush the top, and thereby to determine the relative values of the resisting forces of extension and compression, and thus to furnish the requisite data for the deduction of formulæ for computing the strength of tubes of any required dimensions. The experiment described in the following letter supplied these data, and fully established the proportional areas of the top and bottom sides of the tube.

By this experiment, a laborious scientific research was brought to a close, which, it is to be hoped, may prove useful in the future requirements of the arts.

MY DEAR SIR, Millwall, April 17th, 1847.

I have more than ordinary gratification in communicating to you one of the most satisfactory experiments that has yet been made in this, of almost all others, most interesting inquiry. It will be fresh in your recollection, that our last experiment, although conclusive as regarded the strength of the bottom of the tube, was nevertheless defective, the tube having broke at about one-half the distance between the weight and the support. This deficiency arose from the unequal distribution of the material, which, on strengthening the bottom with new plates, had not been carried beyond the point of fracture, or only about 20 feet on each side of the load. In the recent repairs of the tube, this defect was remedied, by the addition of a thinner plate being riveted from A to B on each side, thus :—

Fig. 47.



equalizing the strength of the bottom, as nearly as possible, to what it should have been in the last experiment, or in the ratio of the strengths on each side of the centre towards the supports. This arrangement and distribution of the material having been completed, we again commenced the experiments on Thursday last, and have continued them up to the present hour.

On Thursday we laid on about 61 tons, and took the deflections regularly as before; yesterday we continued adding to the weights until 189,170 lbs. were laid on, which were nearly 15½ tons more than the tube ever supported on any former occasion. Fearing there might be some anomaly from the unexpectedly great increase of strength, and finding the mass of weights did not move so freely as I could wish, we discontinued the experiment late last night, and this morning had the ground excavated all around, in order to be quite certain that the tube had the whole weight upon it. This was proved beyond a doubt, and after restoring the weights, which we removed, it ultimately gave way by *the crushing of the cellular top with a load of* 192,892 lbs.=86¼ tons, and a deflection of 4·88, or nearly 5 inches.

This experiment relieves my mind (as I trust it will do yours) from any doubts as to the proportions, and as to the security of the vast structure on which we are engaged. We have now data on which we can safely depend, and it only remains for the mathematician to supply the necessary formula, for calculating the strength of any other description of tube.

Mr. Clarke will go into this inquiry, and I have no doubt he will render the same valuable assistance in this, as he has done in every other investigation with which he has been connected.

I think it will be desirable that you should see the fracture. The tube will remain in its present state, and I hope you will come down some day next week, and let me have your opinion as to the result. You will observe that the top and bottom areas are now in the proportion of 24 to 20, or as 6 to 5*.

I am leaving for Manchester this afternoon by the Express, and remain, my dear Sir,

Yours faithfully,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

P.S. The sides of the tube behaved admirably.

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24 Great George Street, Westminster,
April 28th, 1847.

MY DEAR SIR,

I am requested by Mr. Robert Stephenson to express his regret, that he is so overpowered by Parliamentary business that he cannot reply to your letter at length, and he bids me press upon you seeing Mr. Clarke (whom he has sent to Conway), and arranging to have the proposed change in constructing the tubes on the spot carried out, for which it will be necessary to have a special Board called, which he trusts you will have done, and devote a few days to them and this matter in London.

Yours truly,

William Fairbairn, Esq. J. E. SANDERSON.

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24 Great George Street, Westminster,
June 8th, 1847.

MY DEAR SIR,

I am much gratified at your resolution, to devote a considerable portion of your time to looking the tube-builders up, and getting a good job made of the whole affair. I do not think that a large proportion of your time is at all necessary. Something like two or three days a fortnight is, I am sure, all that you need give up to it; what would be most valuable is a regular periodical visit, so that the progress may be narrowly watched, and advantage taken of every new continuation as it occurs. Of these there will be many which must suggest improvements in our arrangements. There is a meeting of Directors tomorrow, which I have only received notice of this moment. I will explain what your intentions were at my suggestion, and postpone the matter until the next meeting.

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* It was afterwards ascertained that the relative proportion between the area of the cellular top and that of the bottom plate was as 24 to 22.

Yours faithfully,

William Fairbairn, Esq. ROBERT STEPHENSON.

P.S. Mr. Clarke mentions the engines for the builders. Let me suggest that you arrange with the parties who are to use them, as the Board are only cognizant of one being ordered, or rather submitted to them in your inventory. Let us take care and avoid coming into collision with them again on this point. I am especially anxious on your account, as well as my own, after what has passed. I am sure you will understand the object of my naming the subject in this manner.—R. S.

The punching, fitting and riveting of different portions of the tubes had been in operation for some time at Conway and the Straits; and as everything depended upon the security and soundness of the rivets, Mr. Stephenson was more than usually anxious about them. The following letter was written, for the purpose of giving him additional confidence in the workmanship.

It has already been noticed, that the experiments suggested a new form of girder bridge calculated to sustain severe strain, and better adapted for spanning wide rivers and deep ravines, without the support of intermediate piers, than the form of bridges hitherto employed. The first bridge on this new principle of construction had just been opened at Blackburn in Lancashire; and the comparatively slight deflection, which took place during the passage "of ballast waggons," fully established its efficiency. In the two following letters, this form of girder bridge is again recommended to Mr. Stephenson in preference to a trussed girder bridge, or to any other form having auxiliary means of support*.

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* See Letter to Mr. Stephenson, p. 27.

My dear Sir, Manchester, June 9th, 1847.

I have to acknowledge the receipt of your letter of yesterday, and I shall be glad to meet Clarke at Liverpool in the morning. As stated to you in London, I have made up my mind to devote my best energies to the construction and due completion of the tubes, and I will watch narrowly and regularly the progress of each construction, that the work be well done, and free from blemish in every respect. Such is my determination, and in case of neglect in other quarters, the fault shall not be on my side.

After I have carefully inspected the whole of the works at the Straits and at Conway, I will report to you in full; and now that I have them altogether, I purpose being there once a fortnight, or if necessary, once a week, until they are all started and in full operation.

A report of each man's work will therefore be presented to you every two weeks; and in case you deem it expedient, the same may be handed over to Mr. King for the information of the Directors. More than this I think cannot be done, and I apprehend more than this will not be required.

I have just returned from Blackburn, and although the hollow girder bridges, which I have been making, are not nearly so good as those I am now constructing, they nevertheless contain so many elements of strength, cheapness and security, as to be well-entitled to your consideration.

I am, my dear Sir, faithfully yours,

Robert Stephenson, Esq. William Fairbairn.

The following is an extract from a letter, dated June 10th, 1847, addressed to Mr. Stephenson :—

"I have carefully considered the principle of the trussed girder bridges, and I must honestly confess that my opinion of their strength and security is unfavourable. I have always entertained objections to the trusses, equally so in this form of bridges as I do to chains, or any other auxiliary as a support for girders of any kind. My confirmed opinion is, that every girder should be deprived of assistance of any kind, and should be entirely dependent upon itself for support, and its power of resistance to the load. I may be wrong to some extent in these views, but I think, when we are again together, I shall be able to prove, not by mathematical demonstration, but by some practical facts, that I am correct. I think you will find, that trusses and chains seldom or ever harmonize with rigid bodies, and are more apt to do mischief than add to the strength of the body they are intended to support.

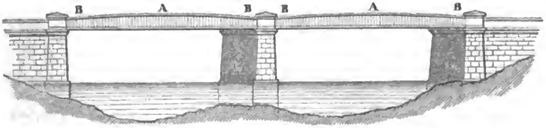
"I find that the plate-iron hollow girder will not only be much stronger, but ultimately cheaper than any other that you can adopt. I have now discovered an excellent principle of construction, one that unites the elements of strength with the economy of material, and which I apprehend will be well-worthy your attention in the construction of bridges of this kind.

"In the construction of the hollow iron girder, the greatest difficulty I have experienced is in making a substantial and satisfactory provision for the cross-beams of the roadway. This I have now accomplished satisfactorily to myself, and I trust, when fully developed and worked out, it will be equally so to the profession at large.

"The construction of the bridge we are now making for Mr. Bidder has suggested this arrangement; and in case you should adopt the wrought-iron girder, I would strongly recommend it for consideration."

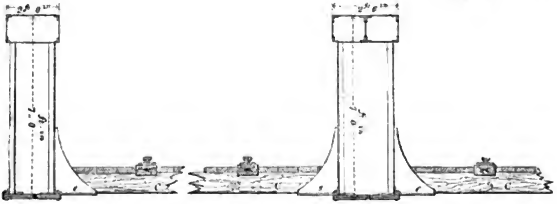
In the same communication the following sketch of the proposed bridge was given for consideration, taking the span of 100 feet, and after referring to some particulars, which do not bear directly upon the subject, the letter proceeds as follows:—

Fig. 48.



"The depth of the girders in the middle at A to be 7 feet, and at B, B, 5 feet. The sections to be as under.

Fig. 49.



"The double plates *a*, *a* to form the bottom of the girder in the usual way, and the plates *e*, *e*, *e* to form a shelf for receiving the cross-beams *c*, *c*, and at the same time to answer as a covering-plate for the joints of the plates *a*, *a*, and also as a tensile plate for the bottom of the girder. From this rough description, you will perceive that the plates *e*, *e*, *e*, *e* run longitudinally along the bottom, and are riveted to it by the angle-iron, forming with the sides a continuous shelf, and receiving the cross-beams at every 3 feet, over which are placed the rails; this arrangement will, as shown above, save considerable expense and weight for the roadway. I trust, from this rough and hurried description, you will clearly understand and approve the principle of construction.

"It adds considerably to the strength of the girder, without much increase, if any, to the weight.

"Besides, it renders the construction of the roadway convenient and secure; and by introducing a cross plate along every third or fourth cross-beam, and riveting this to the outer bracket of angle-iron, every purpose will be served for keeping the girders straight, and retaining the whole in its place.

"In the event of your adopting this description of bridge, I would fix the girder A upon the piers, and by resting the extreme ends at B, B, upon plates with planed surfaces, the expansions and contractions will be effected without risk of distortion in the girders.

"The bearing powers of these girders in their centres would be for the outside ones 182, and for the middle ones 276 tons. This would give, for the bearing powers of one span, and for one line only, a resistance equal to 728 tons equally distributed, or in the ratio of 728 to 90, being 638 tons greater than the heaviest load.

"I am sure you will pardon me for offering these suggestions, and trusting this may be useful."

The next two letters refer to Mr. Stephenson's visit to Conway, his inspection of the tube, and the quality of the workmanship. These letters further allude to the Blackburn and the Camden Town hollow girder bridges, showing their relative deflections and comparative powers of resistance.

My dear Sir, Manchester, July 21st, 1847.

I trust you found everything going forward to your satisfaction at Conway and the Straits. I am getting on rapidly with the drawing for all the beams, plates, &c. for the bridges. I am satisfied they are now strong enough, and I think, when you have a copy of the drawings, they will receive your sanction and approbation. The castings for the Conway are now all complete, and those for the Straits will be put in hand immediately. I shall be there on Monday, and will see Mr. Ross or Mr. Bennet, and explain everything to them.

We have had the trains over the Bolton and Blackburn Bridges, and they answer admirably. Three heavy engines, linked together, were driven over them at high and slow speeds, with a deflection of only $\frac{4}{10}$ths of an inch in the one, and $\frac{3}{10}$ths in the other. They are perfectly rigid, and although not so well-proportioned and well-made (Garforth made a mistake in them) as those I am now constructing, they are nevertheless of the most satisfactory description. I must send you all particulars on the first opportunity; till then I am faithfully yours,

Robert Stephenson, Esq. William Fairbairn.

24 Great George Street, Westminster,
My dear Sir, July 22nd, 1847.

I found everything going on well at Conway. I like the substantial appearance of the whole thing. The sides are much stiffer than I anticipated. I examined the holes and riveting closely, and I thought it well to order that the rymer should be put through every hole. This may not be necessary in every case; but as it is difficult to draw the line where the rymer should or should not be used, I thought it the safest plan to give an order that all should be done.

I am glad to hear that the tubes on the Bolton line are so satisfactory. The deflection you name is greater than I should have expected. Our

Chalk Farm tubes had 150 tons upon them, without any perceptible deflection. This was Mr. Dockray's report to me.

Yours faithfully,

William Fairbairn, Esq. ROBERT STEPHENSON.

Before casting the large beams, A_1 and A_2 (see fig. 50), intended for supporting the hydraulic pumps, and for sustaining half the weight of the tube, Mr. Stephenson considered it desirable to have the opinion of Mr. Hodgkinson as to their strength. This was accordingly done; and after visiting the foundry and inspecting the patterns, he suggested that a number of ribs, *a*, *a*, *a*, *a*, should be used as supports for the top and bottom flanches of the beam. These connecting ribs were accordingly introduced; but most of those cast from the improved pattern broke, as I anticipated, in the contraction of the metal. Subsequently they were reduced to the shape of brackets, which rendered the casting perfectly sound.

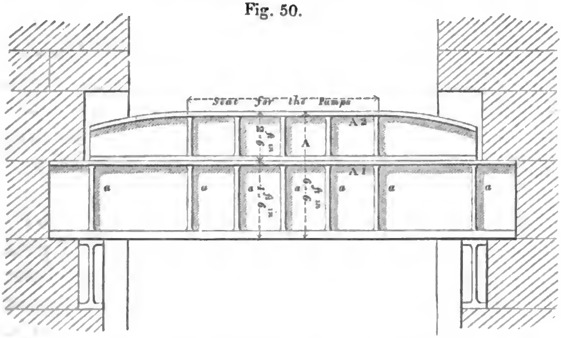
The breakage of the large beams from unequal contraction caused great uneasiness; and in order to ensure safety in a structure of such vast importance, the top beam A_2 was introduced, and by giving an equal distribution of the pressure upon the large beam, the utmost security was attained for the lifting of the Conway tube.

MY DEAR SIR, Manchester, July 23rd, 1847.

I find I shall have to make some alterations in the large cast-iron beams for supporting the water presses. Two of the others have broken in the cooling; and I am now going down to Warrington in order to make some change in the proportions. I find I shall have to use double beams, as under, for supporting the pumps.

This will cause a more equable distribution of the load upon the lower beam, and render it secure and free from risk. With the simple beam, presenting such anomalous results in the shrinking, we should not feel safe. The law of crystallization seems to vary considerably under nearly the same circumstances; and although the same metal is used, and everything precisely similar, it is anomalous that five or six of the beams should be perfectly sound in their contraction, and that two of the others should crack in the middle without any apparent cause. These are the results of the beams which are built in the wall, and with your permission, I will not only make some alteration in those beams, but use the double beam for lifting the tubes.

Fig. 50.



I am glad you like the appearance of the Conway tube: all the others will be equally substantial, and I trust even superior in quality as regards the workmanship. You have done quite right by insisting upon the rymering of the rivet-holes; and I shall support and confirm your orders on Thursday next, when I am over at Conway and the Straits.

I have no doubt the Chalk Farm bridge is much more rigid than those I have made for the Bolton and Blackburn line. It is nearly double the depth as compared with the span, and I dare say a much more expensive bridge. From Mr. Flannigan's (the engineer's) report, they are however sufficiently strong, and appear to give perfect satisfaction. On my return from the Straits I shall, however, have them tested with great care and let you know the results.

Meanwhile, I am, yours faithfully,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

At the period of the date of the following letters, sound riveting was, as it is now, a desideratum in the construction of wrought-iron tubular bridges. At the commencement of the construction of the Conway and Britannia Bridges, it was stipulated that the contractors should use the riveting machine in every case where it could be applied; and in order still further to enhance the value of this process, all the parts which could not be reached by the machine were to be riveted with heavy hammers, for the purpose of causing the rivets to fill the holes, and otherwise to make the work, as nearly as possible, equal to that done by the machine. During the progress of the construction of the tubes for the Britannia Bridge, the machine-work was found (according to the opinion of Mr. Mare the contractor) both expensive and inconvenient, on account of the size and great weight of the plates, and the difficulty of suspending them over the machine. These drawbacks were not however experienced by Mr. Evans, the contractor for the Conway tubes, who overcame every difficulty by the introduction of powerful travelling cranes, which enabled him to rivet the greater part of the bottom and sides of both tubes by the machine. Mr. Mare, the contractor for the greater portion of the tubes for the Britannia Bridge, adopted a different method; and by the use of heavy hammers made the work, if not equal in solidity, at least nearly so, to that done by the machine. The superior quality of the hand-riveting executed by Mare, with the heavy hammers, imposed the necessity of using the same means in the hand-work done by the other contractors, Messrs. Garforths and Evans. All the riveting therefore, not admitting of the use of the machine, was done in the way described, and with much greater effect than could possibly be accomplished by the light hammers generally used in the old process.

As the time drew near for floating and raising the tube, it became a question of some importance, how to attain an effective system of organization of the hands to be employed for the purpose. Up to this time Mr. Stephenson had not expressed himself definitely on the subject; but his letter dated the 23rd

August was sufficiently explicit, and set the question entirely at rest.

MY DEAR SIR, Menai Straits, August 16th, 1847.

I have gone carefully over all the riveting as practised by Mare and Garforth, the one by machinery, and the other by hand. Both of them are good in their way, and now that we have 6½ and 7 lbs. hammers, we can scarcely do otherwise than make good work. I shall see Evans tomorrow on the same subject, and still further increase the size of his hammers.

Now that we have made a beginning here we shall go on with speed; and from the active habits of Mare's foreman, I have no doubt they will turn off a great deal of good work. As soon as one of the tubes is well advanced, we shall see how far it may prove desirable to have the fourth platform constructed. I apprehend it will be wanted, as all the four tubes will be required for lifting at one and the same time. By the by will you write me to Manchester, stating whether it is your wish that I should take charge of the floating and raising of the tubes? I have no objection to do it, *and to take the management of the whole thing, subject to your approval, and to be responsible for the result.* I should however require some alterations in the pontoons, as you will readily remember that I have all along contended for large vessels, and Mr. Clarke on the contrary for small ones. Now, I am of opinion that the process of floating is the most critical and precarious of the whole; and considering the great depth of the tubes, and the height of the centre of gravity, I am firmly persuaded, that the remaining pontoons should be at least 30 feet larger than those at present constructing by Mr. Evans. It is most desirable that this part of the subject should be well considered.

I have no objection, with your consent, to take the management of the whole; and I shall be most happy to have at my command the valuable assistance and intelligence of Mr. Clarke. Will you inform me what are your views on this subject, as no time should be lost in a matter which involves considerations of such deep importance?

I am drawing up an advertisement for giving publicity to our new patent girder. You are aware it has been taken out in my name; and although I have probably contributed much more than most others in determining its proportions, as regards strength, &c., I nevertheless am of opinion, that your name should stand at the head of the invention.

I do this from a conviction that you are entitled to precedence, not from having worked harder, or done more for it than myself; but from the undeniable fact, that I should never have thought of the experiments which led to the discovery, but from the instructions which I received from you relative to the large tubes.

These experiments have unquestionably led to the development of an entirely new æra in the history of girders and bridges, particularly of those of large span. On all these points you must endeavour to steal an hour from your other engagements, and let me know what are your wishes, and I will try to carry them into execution.

I am, my dear Sir, very faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

MY DEAR SIR, London, August 23rd, 1847.

I was surprised at your letter this morning, asking me if I wished you to take charge of the floating and lifting. *I consider you as acting with me in every department of the proceedings*, and I shall regret if anything has been done which has conveyed to you the idea, that I was not desirous of having the full benefit of your assistance in every particular.

Pray therefore, if Mr. Clarke be with you, confer with him as to all that has been done with the pontoons, and then let me see you both. I am going to spend three or four days at Tapton with my father—say from next Sunday until the Wednesday following. I wish very much you and Clarke would join me ; my father will be delighted to see you; I hope therefore you will contrive to come.

I wish in the meantime, you would desire Clarke to order Evans to put decks to all the pontoons he has built, as well as raising their sides one foot. The model which I have here, showing the tube and boats to an exact scale, indicate to me very considerable stability; but to make everything safe, I will at once concur with your notion of increased length. When at Tapton we will discuss your proposed advertisement as to the patent. Your view of the matter seems to me just and honourable.

Believe me, yours sincerely,

William Fairbairn, Esq. ROBERT STEPHENSON.

The following letter indicates the views entertained by Mr. Stephenson, as to the size and strength of the pontoons for floating the tubes. From the first, he entertained the idea that an increased number of small barges would be preferable to a smaller number of large ones. I took a different view of the subject, and contended for the large ones, as being steadier and safer. Subsequent experiments, and the floating of the Conway tube itself, however, confirmed the accuracy of Mr. Stephenson's opinions; as the lighter pontoons proved as secure as those of greater tonnage, and were probably more manageable.

24 Great George Street, Westminster,
No date.

My dear Sir,

I am glad you have settled with the Colebrook Dale Company. These arrangements I leave entirely with you; and you may rest assured I will agree in any arrangement you may deem expedient to make with the contractors.

I have been constantly canvassing and calculating the stability of the barges lengthened, since I left you, without having arrived at any very satisfactory result. The great height of the tube, and the position of the centre of gravity, render the use of sheet-iron barges very precarious; but with long ones the difficulty does not entirely disappear; at least another starts up, although I do not deem it so formidable; that is, in the strength of framing and trussing which long barges must necessarily require to prevent them breaking in the middle. In any case, I have a strong leaning to the employment of a considerable number of vessels instead of a small number; for with a large number, a casualty to any one or two of them would not much interfere with our proceedings or progress during the floating. This, I think you will agree with me, is of the very last importance.

I am compelled to leave for Paris on Saturday, but I shall return as soon as practicable; in the mean time I leave the arrangement entirely in your hands. This evening I shall talk to Clarke about the $\frac{1}{2}$-inch plates. I hardly understand it from your brief allusion to them, but I have no doubt you have explained it to him.

Believe me, my dear Sir, yours faithfully,

William Fairbairn, Esq. Robert Stephenson.

The prognostications of failure which assailed us from almost every quarter rendered every precaution necessary; accordingly, it was agreed, to build two temporary stone piers, and as soon as the tube was finished, to place it upon them free from all other support, and then to test its strength by actual experiments.

As the construction drew near completion, and the first day of trial was at hand, the utmost anxiety was depicted in the countenance of every one interested in its success. A great and important problem had to be solved; and Mr. Stephenson, I have no doubt, shared with me considerable mental anxiety until the tests were applied, and every doubt removed. The ultimate issue of the undertaking involved the loss or gain of reputation, which from the commencement had been staked upon the result. The question repeatedly asked was an important one:—"Will the large tube follow the same law in its powers of resistance as that indicated by the experiments on the model tube?" To me the matter was scarcely problematical, but still there existed doubts; and until the tube was suspended on the temporary piers, and had taken its own weight, it was found next to impossible to divest the mind of some degree of alarm, even in the face of the conviction that all was right.

Amongst other schemes for effecting the tests was one proposed by Mr. Lillie of Manchester, namely, to fill the tube with water to a depth commensurate with the weight to which it should be proved. At first the proposal appeared to be well conceived; but on further reflection, some difficulties presented themselves in making the bottom and sides water-tight, and the admission of the salt-water* between the joints of the plates, which is alluded to by Mr. Stephenson in his letter of December 29th, was a serious objection to the plan. Altogether the scheme, though feasible, was abandoned for the reasons there assigned.

Some doubts were entertained as to the strength and security of the temporary piers, composed of soft Runcorn stone, for supporting the weight of the tube. These piers were however increased in strength, by covering the top of the masonry with large iron bed-plates, which, being united to a covering of solid teak wood 8 inches thick, gave increased security by an equable distribution of the pressure over the surface; and thus all apprehensions were removed as to the safety of the piers.

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* Fresh water could not be obtained at an elevation sufficient to run into the tube.

My dear Sir, Conway, December 24th, 1847.

After careful consideration, I have come to the conclusion on the subject of testing the tube, that it will be preferable in every respect, to have it tested with water. I have examined the whole of the D platform and also the sides; and from what I can see, there will be no difficulty in making it sufficiently water-tight, fully to answer the purpose. I would therefore propose, that a dam formed of planking should be fixed at each end of the tube, with valves or flaps to discharge the water in case of need,—the height of the dam to be 6 feet above the D platform, which, if filled to that height, would give exactly 1000 tons equally distributed over the surface. This is probably too great a test, but it can be reduced to 500 or 600 tons, as may be deemed expedient. I should not however fear the result with even the greater weight of 1000 tons. In order to fill the tube to the proposed height, one or two pumps attached to Mr. Evans's engine will be sufficient for that purpose; and these pumps can be worked at pleasure at any given velocity to retard or accelerate the rate of filling. There appears to me to be many advantages in this mode of testing, such as equalizing the pressure, and bringing all the parts of the tube to their proper bearings; also *the great nicety and regularity with which it can be tested,* and that, as circumstances may require. Entertaining these views, and having a full conviction of the superiority of this method of determining the bearing powers of the tube, I should be glad to have your sanction to its adoption. Mr. Clarke will inform you more in detail as to its advantages, and I have no doubt it will be found to be the cheapest and the best. As soon as I hear from you, I will give Mr. Evans orders to prepare the pumps and the two dams, which are all that are wanted to make the experiment.

I have carefully gone over the calculations as to the pressure that will come upon the temporary piers, and find that under the most unfavourable circumstances, it will not exceed 13 tons to the square foot. You need not be alarmed as to the strain being all upon the outer edges, as the cast-iron cross-beams, and the four lifting beams which are connected by props or pillars with a couple of keys under the ends, will distribute the pressure, as nearly as possible, over the whole area of that part of the tube which rests upon the pier. Besides, it must be borne in mind that we have a good mural platform upon the top of the masonry.

My friend Mr. Lillie is here with me to look at the works, and, like every one else who possesses judgement in these matters, is convinced of the strength and great security of our work.

We shall have one of the cast-iron frames in today, and as everything is now done for one end, I expect to have it finished, or nearly so, by the end of next week. Altogether, we shall be ready, I have no doubt, for testing before the middle of next month, probably about the 10th of January.

I could wish you to accompany me over here about the 4th. I am returning tomorrow, but will be here again next week; and on the Tuesday after I expect we shall be ready for inspection. You will therefore oblige me by dropping a note to Manchester for my guidance, and believe me, always faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

Adelphi Hotel, Liverpool,
December 29th, 1847.

MY DEAR SIR,

I will be with you tomorrow morning to speak about the waterproof of the tube. I cannot say I like it, on account of the salt water filling the spaces between the plates with a thin layer of salt. The chemical effect of this is entirely doubtful. As I shall so soon see you, perhaps at the Polygon this evening, I will not say a word more at present.

Believe me, sincerely yours,

William Fairbairn, Esq. ROBERT STEPHENSON.

About this time Mr. Stephenson expressed a desire, that everything connected with the transport of the tube, particularly the pontoons, which had to float it, should be minutely examined. This was accordingly done; and as the vessels which had been constructed by Mr. Evans the contractor, appeared defective, both as regards workmanship and construction, considerable alterations were ordered to be made; by the introduction of strong bulkheads, decks and tie-bars, and by a thorough recaulking, the pontoons were rendered perfectly secure and fitted for the work they had to perform.

24 Great George Street, Westminster,
January 7th, 1848.

My dear Sir,

Just as I was making arrangements to come down, I received an express from the resident engineer of the Midland, that the Amber Gate Tunnel was moving from some external pressure. I have therefore been down there instead of with you. I am glad to hear however from your note received this morning, that all is progressing satisfactorily, though not with that despatch which could be desired. Your presence will do much, and I hope you will give as large a portion of your time as you can possibly spare. You say nothing about the boats having been tested under the tube. I am very anxious to see this done, for I fear they will require a great deal of stiffening. I cannot now get away from London until after the Board on the 12th.

Faithfully yours,

William Fairbairn, Esq. ROBERT STEPHENSON.

The responsibilities which the testing, floating, and raising of the Conway tube involved, were apparent from the more than ordinary caution that was observed in all the preparations that were going forward for that purpose. The pontoons,, bearing girders, and hydraulic pumps, were frequently and closely inspected; and in order to make everything as secure as possible, additional time was allowed, and hence the tube was not suspended for some days after the date of the following letter.

Professor Babbage, and some other scientific gentlemen, had expressed a wish to be present at the experiment of testing, but the time proved inconvenient for Mr. Babbage, and the trial therefore took place, in accordance with Mr. Stephenson's request, in the absence of strangers.

My dear Sir, Conway, January 21st, 1848.

After a complete investigation of the state of the works here, I have come to the conclusion, with the consent of Mr. Clarke and Mr. Evans, that it will be premature for you to be here before this day week, the 28th inst. We could probably be ready for testing on Tuesday, but it would hurry the thing too much, and I am most desirous that we should have all finished and clear of confusion before we attempt the suspension of the tube. This will be accomplished before Friday next, and I should feel obliged if you will keep yourself disengaged, in order that you may spend the remainder of the week at this place. We shall have three or four days' work for you, as we purpose to test the pontoons, the presses, and large girders at the same time; and I am quite sure it is your wish, as well as my own, that we should have all the work complete, and leave nothing to chance.

It is either to "make a spoon or spoil a horn" with us, and I am most desirous that the "spoon" should not only be well-made, but such as ought to come from the hands of an artist. I believe it will be so, and I trust when you reach us on Friday, that you will pronounce the whole as a good, and for a first essay, a *perfect job*. We have yet a good many odd matters to do, to make the necessary arrangements for floating, raising, &c. Professor Babbage is desirous of being present at the experiment of testing, but I think we had better be entirely alone, and let him visit the tube when the load is on it, some days after we are ourselves satisfied with the results. But I shall see you on these and other matters on Tuesday morning next, as I purpose returning to Manchester tomorrow; and having one day's work in London on Tuesday, I shall see you on the morning of that day, and return to Conway by the Express train the same evening. I therefore wish you to follow on the Thursday, when everything will be ready for your inspection.

I am, my dear Sir, ever faithfully yours,

Robert Stephenson, Esq. William Fairbairn.

It might be considered invidious, if not ungenerous, to impugn the motives of others, in what may be considered a legitimate opposition to the experiments from which the principle as well as the construction of the Conway tube were derived. It does not, however, admit of doubt, that the construction of the model tube was opposed, and the experiments made upon it to some extent condemned as inapplicable, by persons from whom a different course of proceeding might have been expected. Labouring under prognostications of failure, it is perhaps not extraordinary that in the moment of success I should have betrayed some excitement, and have made use of certain hasty expressions, which in calmer moments would not have escaped me. It must be remembered that the testing of the Conway tube was an event of no ordinary kind: it was an experiment upon a gigantic scale, involving results of the deepest interest in relation to the mechanics of engineering. Under the influence of these feelings I communicated the result of the test to several gentlemen, eminent in science, and to others of high standing in the profession.

In making these communications, I, however, disclaim any intention of appropriating to myself the merit of the undertaking, or of hurting the feelings of any person whatever, far less those of Mr. Stephenson. Hastily written notes were sent to Professor Moseley, Mr. Babbage, Professor Willis, Mr. Rennie, and others, but I am not aware that they contained any expression calculated to give offence. It is much to be regretted that these letters were written, as they gave dissatisfaction to Mr. Stephenson, and eventually, in connection with other circumstances, led to my retirement on the completion and erection of the first Conway tube.

All the facts connected with the suspension of the Conway tube on the temporary piers, and the different tests to which it was subjected, were regularly forwarded to Mr. Stephenson; the letter dated February 2nd, 1848, makes a comparison between the experiments made on the full-sized tube and those which had been made on the model; the result of this calculation fully verifies the formula which had been used for determining the proportions of the different parts of the tube.

My dear Sir, Manchester, February 2nd, 1848.

On comparing the experiments on the Conway tube with that of the model at Millwall, I find, that so far as they were carried, its deflections and powers of restoration are as near as possible the same.

The whole of the model tube experiments appear to be borne out by those on the large scale; and there can be no doubt but both tubes follow the same law as regards the deflections, and the elastic powers by which the form as well as the position is maintained. Through a series of distinct experiments, in the increase of load, I find the model tube followed a constant increase of deflections of one-tenth of an inch to every two tons laid on, till fracture took place with 86¼ tons.

Comparing this with the ratio of deflection as given in the Conway tube, I find it would sustain a weight of 2200 tons with a deflection of 30 inches before it broke*. This is with the weight equally distributed over the bottom of the tube, and exclusive of its own weight; the ultimate deflection of 30 inches being however due to the weight of the tube and its load of 2200 tons. If I am correct in my calculations, which I

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* Here let $\delta=$ the ultimate deflection; $\mathrm{I}=$ the moment of inertia of the section of rupture; $h=$ the distance of the upper side of the beam from the neutral axis; $\mathrm{S}=$ the force opposed to compression, per square inch, at the upper side of the beam; $\mathrm{E}=$ a constant, usually representing the modulus of elasticity; the other notation being the same as at p. 68; then we have (see Moseley's Engineering, p. 507),

$$\delta=\frac{l^3\mathrm{W}}{48\mathrm{EI}} \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

Now, assuming that this equality obtains at the point of ultimate deflection, we have (see Moseley's Engineering, p. 554)

$$\frac{\mathrm{W}l}{4}=\frac{\mathrm{SI}}{h} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

$$\therefore \quad \mathrm{I}=\frac{\mathrm{W}lh}{4\mathrm{S}}.$$

Substituting this value of I in equation (1.),

$$\delta=\frac{l^3\mathrm{W}}{48\mathrm{E}}\div\frac{\mathrm{W}lh}{4\mathrm{S}}$$

$$=\frac{l^2\mathrm{S}}{12\mathrm{E}h};$$

similarly,

$$\delta_1=\frac{l_1^2\mathrm{S}}{12\mathrm{E}h_1};$$

will show by and by, we have a sustaining power of 2200 to 250, or in the ratio of 8·8, or nearly as 9 to 1.

Suppose, however, that we have a power of resistance of only 8 to 1, we then have a power which nothing in the shape of a railway train can injure. I have deduced these calculations from the model tube, which broke with 86¼ tons, and a deflection of 4·88 inches.

Will you inform me when you next purpose being at Conway? I was thinking of going over again early next week in order to make arrangements about the pontoons, and to see that they are equal to their work.

I am, my dear Sir, yours faithfully,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

In the following letter Mr. Stephenson asserts that "Mr. Hodgkinson's experiments alone have given the true law that governs the strength of different-sized tubes." Now what this law deduced by Mr. Hodgkinson is, I have yet to learn. For anything which I know to the contrary, Mr. Hodgkinson may have communicated something of the sort to Mr. Stephenson, but if this

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hence by division we have

$$\frac{\delta}{\delta_1}=\frac{l^2}{l_1^2}\cdot\frac{h_1}{h};$$

but in similar tubes,

$$h : h_1 :: l : l_1, \quad \therefore \frac{h_1}{h}=\frac{l_1}{l},$$

$$\therefore \quad \frac{\delta}{\delta_1}=\frac{l^2}{l_1^2}\cdot\frac{l_1}{l}=\frac{l}{l_1} \quad . \quad . \quad . \quad . \quad (3.)$$

hence it appears, that the ultimate deflections of two similar tubes are to each other as their lengths.

Moreover the deflection of a beam, when uniformly loaded throughout, is the same as though five-eighths of that load were suspended from its middle point (Moseley's Engineering, p. 514).

Taking, as in the model tube, $\delta_1=4{\cdot}88$, and

$$\frac{l}{l_1}=\frac{6}{1},$$

then by equation (3.)

$$\frac{\delta}{4{\cdot}88}=\frac{6}{1}, \qquad \therefore \delta=29{\cdot}28 \text{ inches},$$

which corresponds, very nearly, with the ultimate deflection of the Conway tube, given in the above letter.

were the case, I never received any intimation of it. Indeed, as I have already stated, Mr. Hodgkinson refused to assist me with his mathematical knowledge, and hence the Conway tube was constructed from formulæ derived from my experiments alone.

MY DEAR SIR, London, February 7th, 1848.

I only reached London this morning from Newcastle, when I received your previous note, upon which I will speak to you verbally. You allow your feelings to get the better of you respecting Mr. Hodgkinson, and I think improperly; for it is clear, that his experiments alone have given the true law that governs the strength of different-sized tubes. Both your plan and my own for calculating the strength are empirical; but Hodgkinson's experiments, and his deductions from them, give the true law with remarkable consistency.

Respecting the gangway, I would rather in the first instance lay down deals. I am glad to hear you are going down to Conway. You have heard, I conclude, that the lifting-crane has broken down, and broken some of the heavy girder castings.

Clarke I believe is in town, but as soon as I see him I will despatch him to Wales again to meet you.

Yours faithfully,

William Fairbairn, Esq. ROBERT STEPHENSON.

MY DEAR SIR, Manchester, February 10th, 1848.

I will not trouble you with any future observations about the experiments; they have realized your original conception and that is sufficient. I shall not therefore quarrel with Mr. Hodgkinson or any other mathematician who gives the law, but confine myself exclusively to the completion of the work, already so well begun. You may therefore calculate upon my cordial assistance, in bringing the whole to a satisfactory conclusion.

I am, my dear Sir, always faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

On the Monday following the date of the next letter, the tube was raised to its full height; and the hydraulic pumps, which worked admirably, proved themselves fully equal to the task they had to perform. The ascent was slow (about 10 feet per hour), and with the exception of a series of pulsations produced by the action of the pumps, the whole movements were perfectly steady and secure. At the commencement of the ascent the workmen exhibited great caution in venturing under the tube, but before it had attained a height of 4 feet every symptom of fear had disappeared; and, during the time it was suspended on the chains, they worked under it at both ends for two days in succession.

The completion of the Conway tube, and the rigid nature of its structure, gave increased confidence in the construction of the tube for the Britannia Bridge. The height of the latter tube being considerably greater than that of the former, it was suggested to increase the depth and height of the angle-plates, and by these means to stiffen the sides, and render the tube free from "buckle," and secure as respects the retention of form. These suggestions were at once acceded to, and are now incorporated in the formation of the Britannia tubes.

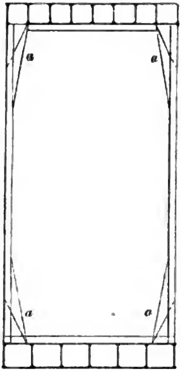
A few days antecedent to the writing of the following letter, a communication from Mr. Bidder appeared in the Manchester Guardian, censuring in severe terms a paragraph which had appeared in that journal, and contending that Mr. Stephenson was alone responsible for the success or failure of the undertaking. That communication, owing to its manner, style, and language, did not seem to require any further reply than that which was given by the editor of the 'Guardian.'

MY DEAR SIR, April 6th, 1848.

I expected to have found you here, as a letter from Mr. Lee informed me that the train would wait your instructions on Tuesday last. Everything goes on satisfactorily, and we expect to have all the raising apparatus complete by Saturday, and ready for raising on Monday or Tuesday next. On either of those days, if not otherwise engaged, we could wish to see you here, but it is not absolutely necessary, as everything appears to be of the best and most substantial character.

There appear to be one or two points in connection with the Britannia tubes which will require attention, and which I have mentioned to Mr. Clarke; the principal of which is to deepen the angle-plates of the four large tubes, so as to stiffen the sides, and connect them more rigidly with the top and bottom cells, as under: that is, to extend a few of the angle-plates, say at every 20 feet double the distance up and down the ┬ iron on every side, as shown at *a, a, a, a.* This would add considerably to its rigidity and retention of form. I am persuaded it is perfectly secure as it is, but we are quite in time to do it, and although not wanted, this would nevertheless make *security doubly sure.*

Fig. 51.



I was sorry to perceive an angry letter from Mr. Bidder to the editor of the Manchester Guardian, complaining of a paragraph published in a previous number, which I knew nothing about, and which with all its inaccuracies was inserted, certainly *without my knowledge.* As stated to you in a previous note, it is not my wish to push myself forward in this matter, and any future notice which may appear in the public prints, you may rest assured does not emanate *either directly or indirectly from me.* I am perfectly content to leave all these matters in your hands, and remain,

My dear Sir, always faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

As every precaution was absolutely necessary in raising the tube, it was deemed expedient, in order to avoid the consequences of accident to any part of the apparatus, to follow up the tube in its ascent with wood packings placed under the bottom at each end. This was at first done with great care, but after attaining the height of a few inches the whole of the machinery appeared so perfectly efficient, as to ensure the utmost confidence in the pumps, chains, &c.; the packings were therefore discontinued, and the tube was raised to the required height, about 20 feet, without the least accident.

MY DEAR SIR, London, April 8th, 1848.

The Board require me in London on the 12th. As soon after that date as practicable I shall come down; in the meantime you will proceed with the lifting, and I rely upon your caution in keeping the packing close up to the tube as it ascends. You will of course arrange for the lifting and the brickwork to go on in steps, as we agreed upon when last at Conway together.

When the paragraph in the Manchester Guardian was shown to me, I said at once that I was confident Fairbairn had nothing to do with it, and declined taking any notice of it. Bidder's letter I have not seen: it was written entirely at his own instigation and responsibility; for, from the first, I have resolved not to interfere with what editors of papers like to write.

Yours faithfully,

William Fairbairn, Esq. ROBERT STEPHENSON.

P.S. Your account of the action of the presses is really delightful. I think we shall go on successfully to the end.

I have now brought down this correspondence to the period when my official connection with the Chester and Holyhead Railway Company, as engineer for the construction of the tubular bridges, may be said to have virtually ceased; and I should willingly have passed over in silence the remainder of the events which transpired, were it not that the completeness of the narrative, as well as the justification of my conduct, demand some explanation. Independently of the regret which I experienced in withdrawing from an undertaking to which I had devoted so much time and thought,—an undertaking fraught with the greatest interest, and which had, as it were, grown up in all its magnificent proportions under my own direction,—I can truly say that the disagreement which took place with Mr. Stephenson is on my part much deplored. But I trust that the reader of the foregoing pages will, at least, have arrived at the conclusion, that I had taken the most important part, in developing, and in giving a practical form to Mr. Stephenson's idea, and also in the superintending the construction and erection of the first Conway tubes. The fact is, I laboured almost incessantly in devising plans, or in watching over the practical details of the work, from the day on which Mr. Stephenson's suggestion was communicated to me, until the close of my engagement; and I can sincerely say, that I was always actuated by the principle of leaving nothing undone which could in any way contribute to the successful accomplishment of the undertaking. Regardless of the prognostications of failure with which the scheme was assailed, and in despite of the opposition of those whose assistance I had solicited, I uniformly advocated the peculiar principle on which the Conway bridge has been constructed.

Such being my position, and viewing the extent of the services I had rendered, it will I think be generally allowed, that it was very natural that I should desire to have my name *publicly* associated with Mr. Stephenson's, as joint engineer for these bridges. Indeed it may very fairly be said, that I might have ventured to claim this distinction, since it had been conferred upon me by the Board of Directors, on Mr. Stephenson's own recommendation*. If, instead of *success* having crowned our efforts, *failure* had unfortunately ensued, would not my reputation have suffered as well as Mr. Stephenson's? The working plans having gone forth with my name alone attached to them, and from my being recognized as the acting engineer, might not the whole blame have been conveniently thrown on me, in the case of failure?

It was not, however, on any of these grounds that I was induced to resign my appointment; for there had not then occurred any opportunity where I conceived it necessary to have my position publicly recognized, and I had always believed, that when the proper time came, Mr. Stephenson would be the first to establish that position and acknowledge the services I had rendered. This recognition was however very shortly afterwards denied me. The first Conway tube having been completed, and the success of the principle established, I conceived that the construction of the remaining tubes simply required a close attention to the system of construction already adopted, and therefore might safely be entrusted to those gentlemen, whose constant presence during the building of the first tube had rendered them thoroughly acquainted with the whole details of the work. By such an arrangement, moreover, the Company would save the amount which had hitherto been paid for my services, and I should be enabled to devote my time to other pursuits, which I had neglected for this work, and which now urgently demanded my attention. This was one reason for my retirement; but what chiefly led me to this decision, was the position assumed by Mr. Stephenson, his public misrepresentation of the position I held under the Company, and his endeavour to recognize my services as the labours of an assistant under his control, and acting entirely under his direction. Had Mr. Stephenson, in his public address, done me the justice to state my independent claim to some of the most important principles observed in the construction of the tubes, I might perhaps have continued my services until the final completion of the whole undertaking; and most assuredly this work would never have come before the public. I now appeal to the preceding pages of this narrative, whether Mr. Stephenson's assertions are borne out by the simple statement of facts. I have overstated nothing,—concealed nothing,—and the reader is left to draw his own conclusions from these facts,—after having become acquainted with the course pursued by Mr. Stephenson, which I will in conclusion concisely relate.

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* See extract from the Minutes of the Board, p. 80.

Upon the completion of the first Conway tube, it was resolved, by the gentlemen and inhabitants of the neighbourhood, to entertain Mr. Stephenson at a public dinner, which should at the same time celebrate the satisfactory conclusion of this great engineering triumph. To this dinner I was honoured with an invitation, which I willingly accepted, as no person could be more desirous than myself of joining in this mark of respect to Mr. Stephenson's character and talents. Some days previous to the dinner I however had an opportunity of seeing Mr. Stephenson in London, and learned from him then, for the first time, that there had been a discussion at the Society of Arts on the subject of the bridges, and that my claims to the merit of the undertaking had been advocated as well as his own. He further acquainted me, that the entertainment at Conway would give him the desired public opportunity of setting the question at rest, and that he intended to avail himself of it. I further inferred, from the tenor of his observations, the position he was likely to assume; and as on such an occasion I would not have been justified in expressing anything in contradiction to his statements, I determined to excuse myself from being present. At the same time I became more persuaded of the propriety of closing my official connection with the Company, as it was evident that our pursuits were not likely to be continued with the same harmony and mutual confidence as heretofore; accordingly the following letters, communicating my determination of resigning, were addressed to the Board of Directors and Mr. Stephenson.

My dear Sir, Manchester, May 16th, 1848.

I have been considering for some time past what further services I could render towards the erection and completion of the Britannia, and remaining Conway tubes. It appears to me that the object for which I was engaged has now been attained, and I should therefore like to have your opinion as to the propriety of my retiring from the situation which I now hold under the Company, as soon after your return to town as possible.

It will no doubt be equally conclusive to you as to myself, that the experiments, drawings, &c. being finished, and the first tube in its place, what follows, with regard to the remaining parts of the bridges, is mere matter of routine, and may safely be entrusted to the care and attention of Mr. Clarke, and my services consequently dispensed with. I shall however, as heretofore, be much guided by your opinion on these matters, as I can assure you it would not be agreeable to me to continue to receive the Company's money unless I could render them in return a proper equivalent.

I am, my dear Sir, very faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

DEAR SIR, Manchester, May 19th, 1848.

Since my letter of the 16th, I have thought further upon the subject of it, and have finally come to the determination to resign my situation as engineer along with yourself, for the Tubular Bridges.

I consider, as stated to you in my former note, that I have now fulfilled my mission. *The great problem has been solved,* and the further progress of the works may be left to others having more leisure than myself. I cannot however leave a work of such magnitude and importance without feelings of the utmost respect, and without an expression of thanks to yourself, for the great and unbounded confidence you have all along placed in my judgement. I entered upon the duties I had to perform with a desire and *a determination to succeed*; and from the commencement up to the present time, I never lost sight of one object, namely, the complete success of the undertaking. The increased and unwearied attention which I have given to the subject, has however grievously encroached upon engagements to which I must now devote my time. I can only say in conclusion, that I shall always be ready and willing to render my best services to this or any other undertaking in which you may be engaged.

I am, my dear Sir, always faithfully yours,

Robert Stephenson, Esq. WILLIAM FAIRBAIRN.

From the 16th up to the 22nd no answer was received from Mr. Stephenson, and conceiving that no reply was intended, the following was addressed to the Directors:—

GENTLEMEN, Manchester, May 22nd, 1848.

Having completed the first of the Conway tubes, and those for the Britannia being far advanced, it appears to me that the objects originally contemplated by Mr. Stephenson, in entrusting to me the experiments for determining the practically best form of a tubular bridge; and your object in the appointment, which committed to me the further developement and working out of this new form of bridge, have been completed; and what now remains with regard to the completion of the works, and which is now a matter of routine, may with safety be entrusted to other hands. I therefore beg to resign the appointment I had the honour to hold under the Company.

I beg to forward a statement of my salary, and other charges up to the present time, which Mr. Stephenson will, I have no doubt, duly certify.

I am, Gentlemen, your faithful obedient Servant,

WILLIAM FAIRBAIRN.

To the Directors of the Chester and Holyhead Railway.

In the course of his address at the Conway entertainment, which took place on the 17th of May, Mr. Stephenson is reported to have made the following observations:—

"I believe it will be expected of me—indeed I should feel it improper if I were to omit on this occasion detailing very succinctly a few facts with reference to the rise and progress of the idea which led to the construction of tubular bridges—because, in doing so, it will not only afford me an opportunity of explaining to you precisely what the origin was, but it will also give me the opportunity of expressing my obligation to those who have so largely aided me in bringing about the result which we are met to commemorate. It is now upwards of six, or about seven years, since I entertained the idea of constructing bridges with wrought-iron plates riveted together. I was called upon,—in a smaller case I admit, but not a very simple one—to construct a bridge authorized by Act of Parliament, but with such limitations that it became a matter of extreme difficulty. All the ordinary kind of bridges were discussed, and I eventually hit upon the notion, and the designs were completed, *for a thin tubular bridge,* although not precisely the same as the present, yet *in principle precisely the same.* That was effectually completed, and answers its purpose, and may be now seen on the Northern and Eastern Railway. From that time, however, to the period of commencing the Chester and Holyhead Railway, the idea fell, or dropped rather, for the time, in consequence of the expense of wrought-iron rather exceeding that of cast. On undertaking the Chester and Holyhead Railway, you will all remember that the original designs for crossing the Conway River and the Menai Strait, were by cast-iron arches of very large dimensions, from 400 to 450 feet span. The execution of the latter work, over the Strait, would have been one, under any circumstances, of extreme difficulty, and would have required the utmost facilities to be afforded by those interested in the navigation of the strait. It is familiar to you all that that project or proposal met with a strenuous opposition, whether reasonable or unreasonable it would be very improper in me to stop here to discuss. But it is sufficient to say that parliamentary powers were granted for the construction of a bridge over the Britannia rock, with such conditions attached to it as rendered it all but, if not absolutely, impracticable. It was then, to use a common expression, that I felt myself 'fairly driven into a corner.' No existing species of bridge was at all applicable under the operation of the Act of Parliament as granted; and it was after an anxious investigation of every possible description of bridge, that it occurred to me, that by *reviving the old notion of seven years ago*, that by extending it, it might enable me to get over the difficulty. Approximate calculations were immediately made, and the result of those calculations were such as to satisfy me of the perfect feasibility of the work. And I well remember, when going into the Committee of the House of Commons afterwards, when a change in the direction of the line was applied for, and when the description of bridge was to be announced, on explaining it to the committee, and giving it in evidence,—I well remember, I say, the surprise and the incredulous glance that I received from all parts of the room. However, I had satisfied myself that the thing was practicable, and I stood by it. As soon as the bill was obtained, and it became time to commence, I obtained the consent of the directors to institute a very laborious, and elaborate, and expensive series of experiments, in order, most thoroughly, to test experimentally the theory I had formed, and also to add suggestions for its full development. It was then that I called in the aid of two gentlemen, eminent, both of them, in their profession, Mr. Fairbairn and Mr. Hodgkinson. They had both distinguished themselves for elaborate series of experiments on cast-iron bridges, and although this was a different material, still from their accomplishments and skill, they were well-qualified to aid me in my research. They heartily went into it, and the result is what you now see under the walls of your venerable old castle. But having mentioned these two names, there is another gentleman that I wish to call to your notice,—a gentleman to whose talents, to whose zeal and ability, from the commencement of this undertaking, I am much indebted; and indeed the full development of the principles of tubular bridges is by no means in a small degree indebted to him—I allude to my assistant, Mr. Edmund Clarke. He has been my closet companion from the commencement of the preliminary investigation; no variation or inconsistency in the experiments eluded his keen perception; he was always on the look out for contingences that might affect the success—though not the principle, still the success—of the undertaking; and he and the other gentlemen whom I have just named, are the three to whom I feel deeply indebted for having brought the theory I first broached to such perfection, and I thus publicly tender them my acknowledgments."

The inaccuracies, both as to facts and dates in this statement of Mr. Stephenson, are very numerous. It simply requires a reference to the short description of the Ware Bridge, p. 113, and to the drawings, to disprove the assertion, "that it is a *thin tubular bridge*, although not precisely the same as the present, *yet in principle precisely the same*;" and it can be easily shown too, that considering the Ware Bridge as a simple girder bridge, it is exceedingly defective in design. Is there anything new in this application of wrought-iron plate girders? As well might it be said that the combination of wrought-iron deck beams, so many years applied in iron ships for the support of the decks, is "a counterpart of the proposed cellular top for the Britannia tubes*." I really cannot but regret that Mr. Stephenson, whose name will be always associated with the grandest bridge that has ever been constructed, should have committed himself in making such an erroneous assertion, as that it was by reviving and extending his original conception of this imperfect structure

* See Mr. Stephenson's letter, p. 114.

at Ware, that he was led to originate the bridges crossing the Conway and Menai Straits.

Mr. Stephenson's remarks further admit of the disingenuous construction, that his scheme was matured before the bill for the Chester and Holyhead Railway was passed by parliament, and before I was consulted, and that he was at that early period acquainted with the present design of the bridge. He refers to the incredulous glances which were directed towards him when the *description of the bridge* was explained to the Committee; and intimates that "it was not until the bill had been obtained, and it became necessary to commence, that he requested my assistance." Now my advice was asked by Mr. Stephenson before his evidence to the Parliamentary Committee was given, and he announced his idea to that Committee, strengthened with more than one opinion of its feasibility. Let the reader turn again to the earlier letters of the correspondence, and he will find of what a crude and dangerous scheme that idea consisted; how totally dissimilar, in form and principle, it was to the present tubular structures, and how slowly Mr. Stephenson was persuaded to give up his earliest conceptions. Again, Mr. Stephenson states that he called in the aid of Mr. Hodgkinson and myself at the same time; now it is essential to the proof of my claims, that this assertion should be explicitly contradicted. It was I, and not Mr. Stephenson, that solicited Mr. Hodgkinson's co-operation*, and this was not done until I had been actively engaged for several months in my experimental researches, and after I had discovered the principle of strength which was offered in the cellular top, and not only proved the impracticability of Mr. Stephenson's original conception, but had given the outline of that form of tube which was ultimately carried into execution.

When Mr. Stephenson had made up his mind to claim, in the manner he did, the whole merit of the undertaking, it is not difficult to understand his reason for giving Mr. Clarke—his own assistant—so prominent a position. I willingly bear my testimony to the great value of the services rendered by Mr. Clarke, to his talents, and to the great energy which he displayed in working out his several duties, but these had no reference whatever to the designing of the structures.

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* See letters, pages 14 and 15.

When the report of the proceedings at the Conway Dinner was made public, Mr. Bateman, C.E., voluntarily came forward to assert my claims; and in a letter addressed to the Editor of the *Manchester Guardian*, pointed out the inaccuracies and misstatements of Mr. Stephenson, to which I have just referred. Mr. Bateman, in his letter, showed the unjustifiable position which had been taken by Mr. Stephenson, and asserted, that in an engineering work of such novelty and magnitude, Mr. Stephenson would not have injured his own reputation, by acknowledging, in suitable and truthful terms, the merits due to those who had rendered him the most valuable service. It is presumed that there will be no dissent from this opinion. Mr. Stephenson replied to this letter, and the tenor of his remarks showed his determination to stand by his public assertion. He quoted, from my letter of the 27th of October, 1846, my testimony to his claim of originality in having the application of *a* wrought-iron tube for the purposes of railway traffic; a great merit which neither Mr. Bateman nor myself had ever denied to him, and which I have uniformly asserted that he is undoubtedly entitled to. But he left entirely untouched the point at issue, viz. that it was almost exclusively my exertions which gave to his conception a useful and practicable form—that the experiments which I had conducted and originated, showed the weakness of the circular tube, which he had originally recommended—that I alone showed him the danger of the principle which he was anxious, for so great a length of time, to carry out, by attaching a flexible catenary to a perfectly rigid platform or roadway—that from the results of these experiments, I designed and submitted for his approval an entirely novel kind of tubular bridge, different in form, different in principle, vastly superior in economy of material and strength, and which was finally approved and carried out, and which is now in existence, spanning the rapid estuary at Conway, and admirably fulfilling the varied requirements of railway traffic with perfect security.

The truth of these assertions will, I confidently trust, be impressed upon the mind of the unbiassed reader of the preceding correspondence and narrative. I hope, moreover, that the whole will be interesting to the profession, by showing accurately the modes of proceeding adopted in carrying out the greatest engineering work attempted in modern times.

In the following sections of the work will be found in detail, descriptions of the construction of the tubes, of the means used for floating and raising these enormous masses to their places on the piers, and of the whole of the preliminary experiments; and I would urge upon every one, interested with the foregoing narrative, an attentive perusal and consideration of them.

It only remains now for me to add, that my resignation as engineer to the bridges was ultimately accepted by the Board of Directors, in terms very different from those of the appointment, showing that they evidently leaned to Mr. Stephenson's statement. I do not consider it requisite to publish this reply to my resignation, as it would simply show how lightly were, at that time, valued services which had rendered the Railway Company incalculable benefits.

PART II.

DESCRIPTION AND DETAILS OF CONSTRUCTION OF THE TUBES.

THE FLOATING AND RAISING AT CONWAY.

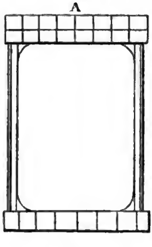
ON THE CONSTRUCTION, RAISING AND FLOATING OF THE TUBES.

THE CONSTRUCTION OF THE TUBES.

The experimental inquiry having determined the proper form and dimensions of the tubes, it was ultimately arranged with the different contractors for their construction, that the operations should be carried on near to the respective sites of the two bridges. Suitable platforms and work-shops were erected by the Company; the contractors fixed the necessary machinery, and in a short time the neighbourhoods of the Conway and the Britannia Bridge were the scenes of busy industry.

The tubes to be constructed had resolved themselves into *huge hollow girders*, each having a series of cells on the top side, arranged so as most effectively to resist the force of compression, and another series of cells on the bottom side. This cellular structure of that part of the tube girder, which would be subjected to a tensile strain, was rendered necessary, as will be afterwards shown, by the practical difficulties which would have been encountered, had it been attempted to obtain the requisite sectional area in a solid mass.

Fig. 52.



In an early stage of the proceedings, it was deemed essential to have a double tier of cells (as shown at A in the annexed sketch) on the top side, in order more effectually to resist the crushing forces, and to supply the required sectional area; and although this form undoubtedly offers an admirable distribution of material, some difficulties of execution became apparent; and, when fortified with the extraordinary result* of the final experiment on the model tube, it was resolved to adopt the more simple construction of a single tier of cells, the proportion of areas being maintained by increasing the thicknesses of the plates and strips.

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* See page 145 and Appendix.

The detail of construction of the tubes for the Britannia Bridge is in all respects similar to that of the Conway tubes, the thickness of the plates, the heights and other dimensions being in both cases in a certain ratio the same, with their respective lengths; the mode of riveting, and the ratio of the sectional areas of the top and bottom, are also in both cases identical.

The principal dimensions of the tubes for the two bridges are as follow:—

	Britannia.	Conway.
Total length of each tube	1524 feet.	424 feet.
Total length of tubes for both lines of Railway	3048 „	848 „
Greatest span in the clear	460 „	400 „
Height of tubes at the middle	30 „	25 ft. 6 in.
Height of tubes at intermediate piers	27 „	——
Height of tubes at ends	23 „	22 ft. 6 in.
Extreme width of tubes	14 ft. 8 in.	14 ft. 8 in.
Number of rivets in one tube	882000	240000
Number of rivets in the whole bridge	1764000	480000

It will be borne in mind that the Britannia Bridge is divided into four spans, the two principal extending from the pier on the Britannia rock to the piers on either side of the straits, and being each 460 feet in the clear, and the spans extending from these smaller piers to the embankments being each 230 feet in the clear. The bearing on the centre pier is 45 feet, on each of the intermediate piers 32 feet, and on the abutments 17 feet 6 inches. The whole length of the Britannia tubes, if placed in a line, would be about three-fifths of a mile. In both bridges the tops have the form of a parabolic curve, and in the Britannia tubes there is also given on the lower sides, a "camber" or rise of from 9 to 10 inches between the points of support on the piers. This additional camber is nearly equal to the deflection of the tube due to its own weight, so that it is expected, when this bridge is completed, that the lower side of the tubes will present a perfectly horizontal line.

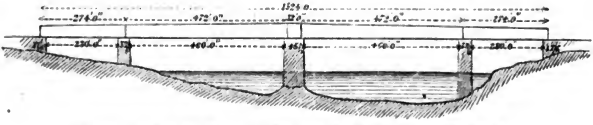
In works of such vast magnitude, it is difficult to ascertain with perfect accuracy the amount of materials consumed in their construction, but the following calculations have been made with the greatest care.

Summary of computed weight of Britannia and Conway Bridges.

	Plates.	Angle-iron.	T-iron.	Rivet-iron.	Cast-iron.	Total.
	tons.	tons.	tons.	tons.	tons.	tons.
1 Britannia tube, 274 feet long ...	450	109	70	60	......	689
3 Britannia tubes, 274 feet long...	1350	327	210	180	......	2067
1 Britannia tube, 472 feet long ...	965	188	139	108	......	1400
3 Britannia tubes, 472 feet long...	2895	564	417	324	......	4200
1 Tube over pier, 32 feet long ...	64	26	10	7	......	107
1 Tube over pier, 32 feet long ...	64	26	10	7	......	107
Frames and beams for Britannia	......	......	......	......	2000	2000
Total weight Britannia ...	5788	1240	856	686	2000	10570
1 Conway tube, 424 feet long......	774	169	109	94	......	1146
1 Conway tube, 424 feet long......	774	169	109	94	......	1146
Frames and beams for Conway...	......	......	......	......	600	600
Total weight of Conway...	1548	338	218	188	600	2892
Total of both bridges......	7336	1578	1074	874	2600	13462

Which gives a grand total of nearly 13500 tons of iron.

Fig. 53.



Sketch showing the principal dimensions of Britannia Bridge.

In round numbers the value of the above total of iron work in both bridges, when fixed permanently, may be stated at £500,000.

Plate VIII. shows accurately the distribution of the material, and the arrangement of the plates in one-half of one of the Britannia tubes, and exhibits also the variations in the thicknesses of the plates in the different parts of the tubes, as they approach and recede from the points of support. These variations will be more particularly dwelt upon by and by; for the present I shall confine myself to a description of the several parts of the tubes, beginning with the

Cellular Top of the Tube.

The top is divided into eight compartments (Plate IV.), and the top and bottom platforms, A and B*, forming these cells, are composed of plates 6 feet long, and 1 foot 9 inches wide throughout, and are $\frac{12}{16}$ths of an inch thick in the middle, and decreasing in thickness as they approach the piers, where they are $\frac{10}{16}$ths of an inch thick. The vertical plates X are of the same thickness, and are varied in strength in the same manner as the platform plates, to which they are securely attached by angle-irons above and below. The plates throughout the top are carefully fitted with their ends abutting against each other, and these joints bound together by double covering plates. The angle-irons at the corner of the cells, which materially assist in the resistance offered to compression, are jointed in the same manner; the rivets in this portion of the structure are all 1 inch diameter, and are spaced 3 inches from centre to centre of hole. The dimensions of the cells are 1 ft. 9 in. by 1 ft. 9 in., and they are sufficiently large to admit the entrance of a man, for the purpose of painting and repair.

The Bottom of the Tube.

This most important part of the structure required the utmost thought in the design, and extraordinary care in the workmanship. Its sectional appearance resembles much that of the top just described, but the method in which it is put together is widely different; and it embodies several important novelties in the arrangement of the plates and system of riveting, to which I gave much anxious consideration, and which were only arrived at after frequent experimental tests. This part of the tube being subjected solely to a tensile or tearing strain, the great aim to be attained in the disposition and arrangement of the materials, was uniformity of strength; that is, the whole bottom was necessarily composed of an infinite number of small pieces joined together, that the joinings of any two of these pieces should approach as nearly as possible in strength to that of the body of the piece itself. It is evident that it was a consideration of much importance, that the number of joints should be as few as possible, and the plates as large as could conveniently be rolled; and after some difficulties we succeeded in obtaining the plates all 12 feet long, or double the length of those used in the top*. The next point was to obtain the required sectional area, and to accomplish this it was found requisite to make the D and E platforms (Plate VIII. figs. 4, 5 and 6) of double plates, which were so arranged that every transverse joint of the upper layer of plates of either platform came exactly over the middle of a plate of the layer beneath; thus (fig. 54); and these joints were again covered on the open side by a plate of the same width, but increased thickness, as those which it came in contact with, as shown at *a b*, fig. 54. The system of riveting observed in the bottom of all well-made tubular girders, is very different from that followed in most other structures of wrought iron, and it is of so much consequence as regards the security and strength of the bridge as to merit a careful description. The tendency of strain on the lower side of the tubes is to separate or open the joints, and on the upper side to force them closer together. It follows, therefore, that in the one case the plates should be most firmly bound together longitudinally, and in the other that the ends should be accurately butted against each other, and only

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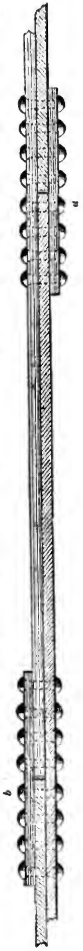
* See also Plate VIII. figs. 2 and 3.

Fig. 54.



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* We were mainly indebted to the great practical skill and attention of Mr. Thorneycroft of Wolverhampton, for the excellence of these large plates.

such a covering plate introduced as would prevent these ends "buckling up" and sliding past one another. The system of uniting the plates of the bottoms of the Britannia and Conway tubes I have denominated *chain-riveting*, from the fact of the rivets being placed one behind the other in the line of the length of the plates, giving them the appearance of a chain. The loss of tensile strength of any plate weakened by the perforation of rivet-holes, is in exact proportion to the transverse sectional area punched out; and the saving of strength gained by the introduction of chain-riveting will be appreciated, when it is stated, that the plates forming the bottoms of the two bridges are only weakened by four holes across each place instead of ten or twelve, which would have been requisite had the old plan been followed. The covering plates on the bottom are all 2 feet 8 inches long, and are of such thickness that the tube is rendered as strong at the joints as it is at any other part. In fact, the chain-riveting has enabled us very nearly to attain a desideratum of equal strength in all parts of the structure; and I believe that rupture would be as likely to take place through the solid body of the plates, as at the joints, when the structure is on the point of yielding. The vertical plates which form the divisions of the cells are all riveted in the same manner. This system is accurately shown in Plate V., which gives a drawing of a part of the bottom of one of the Britannia tubes. The bottom cells are only six in number, and their dimensions are 2 feet 4 inches by 1 foot 9 inches.

The thickness of the plates forming the D platform varies from $\frac{9}{16}$ths at the middle of one of the large spans to $\frac{7}{16}$ths at the ends. Those for the E platform are similar. The thickness of the vertical plates is $\frac{9}{16}$ths in the middle to $\frac{8}{16}$ths at each end. The rivets in the two platforms are all $1\frac{1}{8}$th diameter.

The Sides of the Tube.

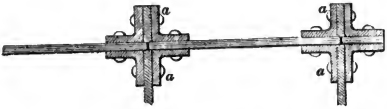
I have always regarded these parts of the structure simply, as the medium of connexion between the top and bottom parts of the tube, which are obviously subjected to the greatest strain. Although not taken into account in the calculation of strength, they nevertheless are too important to be neglected, and require the utmost care and accuracy in their construction. It is necessary that they should possess rigidity, tenacity and lightness.

All bodies, when submitted to a transverse strain, have the top particles compressed into a small compass, and the bottom ones separated wider asunder. The two opposing forces, thus brought into action, become more powerful the further they are removed from the line of the *neutral axis*, a point where the force of compression terminates, and that of extension begins. This neutral line is much more easily fixed and ascertained in wrought-iron tubular girders, owing to the peculiar nature and distribution of their material, than in other bodies having a more solid structure, and composed of more fragile materials. It hence appears, that the depth of a girder is one of its most important features, and that in all such structures the great body of the material should be distributed considerably apart from the neutral axis, and that there should be no more material in the connecting sides than is simply necessary to keep the tube in shape.

In the Britannia and Conway tubes, the sides are, in my opinion, stronger than are necessary. I consider that plates $\frac{5}{16}$ths and $\frac{3}{8}$ths of an inch thick, strengthened as at present with the T iron coverings to the joints both on the inside and outside, would have given sufficient rigidity and ensured the security of the bridges, and indeed the original designs contemplated these proportions. The side plates were however subsequently used nearly double these thicknesses, owing partly to the suggestions of Mr. Hodgkinson, and partly to the very proper consideration, that in a new and untried structure, the disadvantages arising from the increase of weight were as nothing compared with the certainty of strength and security of the undertaking. It was very properly determined, that no part of the work should be deficient, and that no risk should be incurred, even though a few additional tons of material should be used.

The sides of the tubes in both bridges are composed alternately of three and four plates in depth, 2 feet wide, and varying in thickness from $\frac{8}{16}$ths of an inch in the middle to $\frac{10}{16}$ths as they approach the points of support (see Plate IV. fig. 2). At distances of about 10 feet from the piers, the side plates are however again increased in thickness, and much stronger T irons, or rather pillars of that form, composed of a thick plate and strong angle-irons, are riveted over the joints, as shown in the annexed sectional sketch at *a*, *a*.

Fig. 55.



This arrangement and distribution of the material is obviously necessary, on account of the ends on the piers having to support the enormous weight of the tube. In addition to these precautions, massive cast-iron frames are introduced at the bearing points on all the piers, which tend much to keep the tubes in form, and to give great solidity to the whole structure. These cast-iron supports, and the manner in which they are attached to the tubes, are accurately shown in Plates I. and II.

The T irons are firmly riveted over the joints, with 1-inch rivets, 3 inches apart; and the ends of the plates, or cross joints, are "butted" and covered with plates, in the same manner as those on the top cells. The drawings show the manner in which the interior T irons are bent round in order to get a firm hold of the top and bottom sides of the tubes. See sectional views in Plates IV. and VI.

THE FLOATING OF THE FIRST CONWAY TUBE.

The transport of a huge mass of iron 412 feet long, 25 feet 6 inches high, 15 feet wide, and weighing not less than 1300 tons, was a task of no ordinary difficulty. No former effort with which we are acquainted, can, I think, be said to have equalled it, when the unwieldiness of its form, and the extraordinary natural difficulties to be encountered are taken into consideration. Many of the works of the ancients are stupendous in conception and colossal in dimensions, and it has been a constant matter of inquiry, in what manner a people, ignorant of the mechanical appliances which we possess, could raise structures which have resisted all the inroads of time, and which are to the present generation objects of awe and admiration. In more recent times the transport of the immense granite block*, which forms the base of the statue of Peter the Great at St. Petersburg, was looked upon as a most extraordinary achievement, but it cannot be said to have been so formidable an undertaking as the moving of the Conway tube. The granite block was a compact mass, being 42 feet at the base, 21 feet thick, and 17 feet high, and capable of being moved on rollers, &c. to the raft which carried it down the Neva to the site of the city; but in the case of the Conway tube, after the most anxious consideration, and when numerous schemes and proposals had been weighed, examined and rejected, that of floating the mass on pontoons or barges was decided upon as the most feasible and most secure, the centre of gravity being, in this case, necessarily raised several feet. In addition to this disadvantage, the whole had to be handled and manœuvred, in probably the most difficult tideway in Europe, where the current rushes through a narrow gorge of great depth to fill the broad expanse of the in-land bay, at a rate of six to seven miles an hour; and the utmost nicety had moreover to be observed in bringing the tube to its place, as there was only a clearance of 12 inches; that is, the distance between the opposite masses of masonry was only 12 inches greater than the length of the tube. All these obstacles may well be termed formidable; and I therefore conceive that the utmost praise is due to Mr. Stephenson, for the admirable arrangements and contrivances, which rendered the first attempt at so gigantic an operation perfectly successful.

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* This large block, weighing 1500 tons, was transported from a morass to the banks of the Neva, a distance of five miles, and then embarked in a vessel or pontoon prepared for the purpose. From thence it was floated down the river to the spot where it now stands.

For the construction of the tube, and to afford facilities for its transport, it was necessary to select a site as near as possible to that of the intended bridge. It was further necessary, to make a selection suitable for the construction of a platform, which would admit of the entrance of the barges or pontoons, which were intended to carry the tube to its destination; after careful inspection, the place fixed upon was a point between high and low water marks, at a distance of about 300 yards from the site of the bridge. The line of beach is there composed of schistus (slate) rock, affording a perfectly solid foundation, and capable of sustaining any amount of weight. When the first Conway tube was completed, and before the platform or scaffolding on which it was constructed was removed, a temporary stone pier was built under each end at the exact distance of the span, 400 feet, and upon these piers, after removing all intermediate support, the tube was suspended. In order still further to test its soundness and strength, 300 tons of iron were placed as nearly as possible in the middle of the tube, and the observations made during this experiment will be found hereafter recorded in the Appendix*.

In building the piers, which were to support the bridge, the masonry was so arranged that a solid shelf of stonework was left, at a height of about 3 feet above ordinary high water-mark, to receive the ends of the tube on its arrival from the platform, for it was obviously necessary that the transport should be made just before high water, in order that the ends might, when afloat, be above the level of the shelf, and that as the tide receded the pontoons would leave the tube*. Openings were left in the sides of the piers to admit the tubes, and of course that furthest from the site of the platform was fixed first.

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* See experiment in the Appendix.

The pontoons were constructed of wood, and were six in number, each being 100 feet long, 25 feet beam, and about 10 feet deep; they were of rough construction, being intended for temporary purposes, but in other respects they were perfectly substantial, and well-secured with proper kelsom, stays, braces, and were all covered with water-tight decks. They jointly offered a displacement of 2400 tons, but the depths of the sides, flat bottoms, &c. gave them a sufficient buoyancy with a comparatively small draught of water. The haulage of the tube was effected by means of capstans fitted on board the pontoons, and no kind of steam power or towing was used during the operations. The whole of the mooring-chains and hawsers, &c. were admirably distributed by Captain Claxton, R.N., of whose valuable services Mr. Stephenson availed himself, and who was throughout most indefatigable in his exertions.

The first Conway tube was floated to its destination on Monday, March 6th, 1848. The pontoons were floated under the tube early in the day, three at each end, and the hawsers having been properly attached to the several capstans and mooring-chains, the whole was ready for moving as soon as the tide was found sufficient. The weather was most propitious, and a fine spring tide running up the estuary of the Conway; the whole was under weigh soon after eleven o'clock, and in less than three quarters of an hour the tube was safely landed on the abutments. This fact alone will speak for the care bestowed and the completeness of Mr. Stephenson's arrangements; but it will be readily conceived that it was a moment of intense anxiety and suspense, when it was shouted that "it was moving," and all will understand how narrowly every movement was watched until safety was certain, by the touching of the ends of the tube on the masonry. Then the whole of the immense concourse of people assembled gave some hearty cheers, expressive of their delight and satisfaction, and all who had felt the weight of responsibility were deserving of the congratulations which they received at this successful accomplishment of their plans. It may again be asserted, that this hazardous and difficult process of floating was executed in a most masterly manner.

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* The pontoons were all provided with valves, which allowed water to be admitted for sinking them the moment the tube touched the shelves.

THE RAISING OF THE TUBE.

There only now remains for me to describe the last important, and fortunately equally successful operation of raising the tube to its final position,—one attended with some risk, and requiring no small degree of skill and resolution to carry into effect. The suggestions and the means devised for this purpose may be considered as the joint production of Mr. Stephenson, Mr. Clarke and myself; for it will be remembered, that in describing the process which I recommended to Mr. Stephenson, it appeared that a similar idea had occurred to himself about the same time*. My first notion was to apply the hydraulic power on the underside of the tubes, and having elevated them at each end to a height corresponding with the length of the ram, then to fill up the recess by building up the masonry under the tubes, and thus by a succession of steps and lifts, to raise them to the required height.

This process was undoubtedly applicable in the case of the Conway Bridge, where the final position of the tubes was not more than 18 feet above the high-water level; but at the Britannia Bridge, where an elevation of not less than 120 feet was required, it was obviously tedious and objectionable. After several consultations it was finally agreed to use powerful chains, and to fix the hydraulic pumps on the tops of the piers. This point being determined, plans were immediately prepared, and the arrangement, which was suggested, is shown in the accompanying sketch, where there are two hydraulic pumps, B B, resting on the large cast-iron girders A¹ A², which were proposed to be firmly built into the masonry. A powerful cross head, D, connected the heads of the two rams of the pumps, and to this cross head was to be attached a single chain, C, of immense strength, intended to lay hold of each end of the tube exactly in the middle of the large cross beams, as shown at G G, &c., Plate I. and II. The two presses were to act simultaneously; but in lieu of this arrangement, it was suggested by Mr. Stephenson and Mr. Clarke, and finally carried out, to employ a single and larger hydraulic pump with two sets of chains. This apparatus is shown in the drawings. But another great difficulty remained to be overcome, and it was one which presented itself to my mind with great force, viz. in

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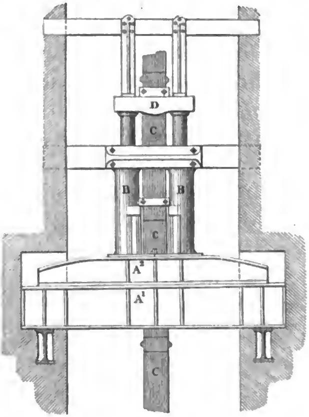
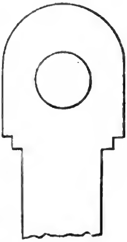
* See letters, pages 92, 93 and 94.

Fig. 56.



what manner the enormous weight of the tube was to be kept suspended when lifted to the height of 6 feet, the proposed travel of the pump, whilst the ram was lowered and again attached for the purpose of making another lift. Much time was occupied in scheming means for accomplishing this object, and after examining several projects more or less satisfactory, it at last occurred to me, that by a particular formation of the links, we might make the chains themselves support the tube. I proposed that the lower part of the top of each link (immediately below the eye) should be formed with square shoulders cut at right angles to the body of the link thus (fig. 57), and as shown at T, Plate II. When the several links forming the chain were put together, these shoulders formed the bearing surface or "hold" for the cross head C attached to the top of the ram B of the hydraulic pump. But the upper part of this cross head was moveable, or formed of "clips," *i*, *i*, *i*, *i* (Plate II.), which fitted the shoulders of the chain, and were worked by means of right- and left-hand screws, as shown in the drawings, and could be either made to "clip" the chain immediately under the shoulders, when the ram of the pump was down, and a lift about to be made, or be withdrawn at pleasure. Attached to the large girders A_2, were a corresponding set of sliding "clips," which were so placed and adjusted as to height, that when the ram of the pump was at the top, there was a distance between the two sets of "clips" equal to twice the length of the travel of the pump, or the length of two sets of the links of the chain. To render the action of the apparatus more clear, suppose the tube resting on the shelf of masonry M, Plate II., in the position that it was left in, after the operation of floating was completed and the chains attached, and everything ready for the first lift, the ram of the pump being necessarily down. The upper set of clips, *i*, *i*, *i*, *i*,

Fig. 57.



attached to the cross head C are forced under the shoulders of the links, and the lower set of clips, *i*, *i*, *i*, *i*, attached to the frames resting upon the girders A_2 are drawn back, so as to be quite clear of the chain; the pumps are put into action simultaneously at both ends of the tube, and the whole mass is slowly raised until it has reached a height of 6 feet from its original resting-place. The "clips" attached to the cross head C have so far been sustaining the weight, but it will be observed that by the time the pump has ascended to its full travel, the square shoulders of another set of links have come opposite to the lower "clips" on the girders A_2, and these clips are advanced under the shoulders of the links, and the rams being allowed to descend a little they in their turn sustain the load and relieve the pumps. The upper clips being withdrawn, the rams are allowed to descend, and after another attachment a further lift of 6 feet is accomplished, and thus by a series of lifts any height may be attained.

The fitness of this apparatus for its work was admirable, and the action of the presses, as Mr. Stephenson termed it in his letter to me, delightful*. At the commencement of the raising of the first Conway tube, it was discovered that a violent pulsation was occasioned, from the simultaneous action of the steam-engine pumps at each end. These pulsations produced an undulating motion on the suspended tube to the extent of a deflection of 1·25 to 1·4 inch, a deflection equivalent to the effect of a weight of nearly 120 tons laid on and taken off at each stroke of the pumps. This alarming action continued to be multiplied, until the speed of one of the engines was reduced, and the strokes of the pumps rendered alternating.

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* See letter, April 8th, 1848, p. 169.

DESCRIPTION OF THE PLATES.

Plate I. exhibits two sectional views of the masonry of the piers and the apparatus for lifting the tubes. Fig. 1 represents a transverse section at the end of one of the tubes, and the hydraulic pumps, chains, &c., which are common to both; and fig. 2 represents a transverse section at the middle of the tube, the architecture of the abutment and the appearance of the bridge when finished.

Plate II. is a side view of the cylinder and ram, and from 40 to 50 feet in length of the tube, including the rollers and cross beams for supporting the ends upon the bed plates and the brass balls above.

In the sectional view, Plate II., it will be observed that the hydraulic press H had to be supported upon four large cast-iron beams A_1, A_2. The two principal beams marked A_1 were 4 feet deep, and calculated together to support a load of 1400 tons in the middle*, or 2800 tons when distributed over their surface. To effect that distribution, and to make allowance for any unforeseen defect in the castings, two other beams, A_2, A_2, each 2 feet 6 inches deep, were laid upon the top of the beams A_1, and by means of a lining of soft wood between them an equal distribution of the load was obtained. This arrangement for the distribution of the weight over the surface of the cast-iron bearing girders gave ample security, and raised their powers of support from 2800 up to nearly 3600 tons, more than three times the actual weight of the tube†. These precautions were however absolutely necessary, as the large beams had to sustain the shocks caused by these undulations, already described, and hence the necessity of their superior strength.

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* Each of the A_1 beams was calculated to a breaking weight of 700 tons in the middle, or 1400 tons equally distributed over its surface.

† The greatest deflection of these girders was only ·108 inch, when the tube, chains, presses, &c., weighing 1360 tons, were suspended from them.

On the top of the ram B was placed the cross head C, having a perfectly flat surface on the top side, and two rectangular openings at each end to admit the chains T, T passing through, as shown at *a*, Plate II.; also two circular holes at *b*, *b* moving upon the slide-bars *c*, *c*, which were attached to the cross beam D to keep them steady.

To each end of the tube three strong cast-iron frames, E, E, E, &c., Plates I. and II., were riveted, and to these again were fitted the transverse beams F, F, F, &c. collectively, forming three strong frames of the height, width, and interior dimensions of the end of the tube. These frames had a double duty to perform,—1st, to stiffen the sides, top and bottom of the tube, and to connect them with each other at the point where it rested on the piers, and had to sustain a pressure of from 600 to 700 tons; and 2nd, to receive the cross beams G, G, to which the chains for raising the tubes were attached.

These latter beams were computed to sustain a load of 3000 tons and upwards; and the more effectually to equalize the pressure upon them, wrought-iron keys were inserted at *e*, *e*, *e*, *e*, between the cross beams and the shoulders of the side frames E, E, which also had the effect of regulating and rendering uniform the tension of the chains. By these means was obtained a power of giving to each beam its equivalent share of the load.

In giving rigidity to each end of the tube three distinct objects had to be accomplished:—1st, to strengthen the sides at those parts, and to protect the upper and lower cells from injury; 2nd, to strengthen the lower cells and to render them adequate for sustaining the weight of the tube when resting wholly upon its base; and lastly, to make the whole of these parts of sufficient strength to resist the strain, whether arising from a vertical pressure upon the bottom, or from a tensile strain when suspended by the chains and hydraulic pumps.

All these points had to be carefully considered, and hence follows the insertion of the strong cast-iron frames E, E, &c., which acted alike as pillars and suspenders, and the cross-beams F, F, &c., which distributed the pressure along the under and upper surface of the cells, thereby equalizing the strain in almost every direction over the interior as well as the exterior portion of that part of the tube.

The cast-iron frames *d*, *d*, *d*, *d*, &c. which embrace the vertical divisions of the lower cells, extend to a distance of 4 feet beyond the bearing of the tube, where they terminate on the sides of the plates, with a thin edge like a wedge, as shown at *g*, Plate II. By this arrangement a strong and perfectly rigid basement was introduced at the points of greatest pressure, without obstructing the entrance into the cells, or in any way impairing the efficiency of the tubes.

In the upper cells, which are not subject to pressure, the same precautions were not required, since it was found that the top cross frames, F, F, were amply sufficient to protect these cells from injury during the process of lifting.

In addition to the two large beams, A_1 and A_2, for supporting the hydraulic press, two strong beams, R, Plates I. and II., were built into the side walls, on each side of the recess, to prevent the unusual strain of such enormous weight fracturing the masonry, and sliding off in the shape of a wedge. Besides these beams, six other transverse beams were inserted at *f*, *f*, *f*, &c., carrying the cast-iron troughs *l*, *l*, &c. which held the gun-metal balls on which the upper part of the tube was supported. On the back or top of the upper troughs rested the ends of six strong cast-iron beams, *h*, *h*, &c. These beams crossed each end of the tube at a distance of 3 inches from the top, and by means of an equal number of screw-bolts, riveted to the sides of the tube, the required weight could be thrown upon the balls already described. A very slight consideration of the nature and objects of the apparatus will show that the tube was not only supported by the top as well as the bottom, but one-half of the weight, resting upon the balls, gave additional security to the vertical position of the tube, when advancing or retiring from the towers by the expansion or contraction of the metal, as the case might be. At the other end, which was stationary, the same principle of support was employed, excepting only the absence of the balls and rollers, which in this case were not required.

The bottom of the tube at the moveable end was supported on the bed plates *m*, *m*, and between them were two frames containing chilled rollers, 48 in number, and 6 inches in diameter, on which the top plate, and along with it the tube, moved with the same facility as on the balls above.

Plate III. represents a ground plan of the abutments and towers L, M, and gives a view of the top side of a portion of both the tubes, together with a ground plan of the hydraulic press, chains, &c., exhibiting their position, and the machinery by which the tubes were sustained during the time of lowering the ram for the succeeding lift. This apparatus consisted of two distinct sets of clips, one for embracing the links upon the cross-head of the ram during the time of lifting, as shown at C, Plates I. and II., and the other for supporting the weight of the tube on the frame N until the ram and cross-head were lowered to a position for seizing the next link.

These changes were easily effected by the simple process of screwing the clips, which slide upon the cross-head C, close upon the chains, when the tube had to be raised; and by a similar movement of the clips, which slide upon the frames N, they were drawn under the shoulders of the links, and by this means the tube was held suspended until the clips on the cross-head were opened, and the ram lowered for the succeeding lift. This was done in a few minutes by the wheels and pinions *k*, *k*, *k*, which opened or closed the clips by right- and left-hand screws, which working in brass nuts were attached to each block.

From this description will be seen, the perfectly simple and efficient nature of the apparatus, and also the facility with which the top links at O could be moved, or rather dragged forward, along the top of the tower, during the ascent of the tube, without disjointing the links.

Plate IV. exhibits in fig. 1, a section of one of the Britannia tubes, taken within a few feet of the middle pier, and drawn to a scale of a quarter of an inch to a foot. It gives a correct and clear conception of the size of the tube in comparison to the locomotive engine which is passing through it. Fig. 2 is an interior view or longitudinal section of the tube, taken through a line intersecting the tube down the centre, from one end to the other. It will not be necessary to describe these parts, as they have already been under consideration, and require no further comment.

Plate V. gives a correct idea of a part of the bottom platform, showing the covering plates over the joints, the mode of riveting, and the T-irons which extend down the sides, and bend round upon the bottom, as already described. At every fourth rib, or at distances of 6 feet, two strong cross plates, ½ an inch thick and 10 inches deep, extend across the bottom of the tube, and, by means of angle-iron, are riveted to the bottom, and also to the T-irons which run up the sides. These cross plates are inserted for the purpose of stiffening the bottom, and for receiving the timbers *a, a, a,* &c. on which the longitudinal sleepers and rails are fixed, and which form the platform or permanent way.

Plate VI. represents a part of the interior, and cross section of one of the cells of the Britannia tube. It is drawn to a scale of 2 inches to the foot, in order to give a clear and comprehensive view of the connection of the sides with the top cells, and the position of the covering strips, angle-irons, rivets, &c. Fig. 1 is a correct section of the first cell A, and a portion of the second cell B, with the vertical covering plates *a, a, a, a,* which are again shown in fig. 2, at *b*. The angle-irons *c, c, c, c,* &c. run longitudinally along the whole length of the tube; and these, as well as the strips *d*, *d*, *d*, &c., figs. 1 and 2, having to resist compression, are formed, as already stated, of square or butt joints, nicely fitted and firmly riveted to the plates on every side of the cells. The T-iron, *e*, *e*, *e*, *e*, as already described*, covers the joints of the side plates of the tube; they are 2 feet apart, and being securely riveted from the top to the bottom over each joint, they form collectively a series of crosses or columns admirably adapted for stiffening the sides and retaining the whole fabric in form. At each joint, the interior T-iron, instead of terminating with a shoulder under the angle-iron, as shown at *f* on the outside, is bent round to the extent of the two outside cells, as at *c*; and by a double triangular plate C, riveted to each, a series of strong brackets are formed, capable of resisting any twist or strain to which the tube may be subjected. The same connecting brackets are attached to the T-iron at the bottom, but extending to a greater height up the sides, to prevent the lateral deflection of the roadway. Fig 2 exhibits the interior of one side of the top cells, and a part of the interior of the sides of the tube, between the T-irons and brackets.

Plate VII. represents a perspective view of a portion of one of the tubes, resting upon the Britannia tower. It exhibits the divisions of the upper part of the tower, as at A, A, A, and the recesses below, C, C, through which the tubes are raised, by the force of the hydraulic pumps. These pumps are intended to be sustained, and placed upon the large cast-iron beams B B, which are similar in form and strength as those used at the Conway, and marked A_1 and A_2 in Plates I. and II. The wall-boxes D, D, are inserted or built into the projecting parts of the piers on each side, and also in the middle, for the purpose of running three large beams marked E, which, on the arrival of the tube at the proper height, are thrust under it through the cells *a*, *a*, *a*, &c. at each end; and these, along with the masonry of the interior of the tower, constitute the support of the tubes. Upon these beams, and the masonry, are boxes and plates of cast-iron, which form the bed of the tube, fixed permanently in the middle tower, but on the marginal piers, and land abutments, rollers and brass balls are attached, above and below, to admit of the expansion and contraction of the tube, the same as described and shown in Plates I., II. and III. for the Conway Bridge. Immediately below the wall-boxes D D, and the large beams B, which support the weight of the tube, are the longitudinal beams F, F, which are imbedded in the masonry to an extent of 8 or 10 feet from the face, on each side, and prevent the enormous pressure of the tube, when supported from these parts, crushing down the masonry, and endangering the safety of the tube. The short transverse beams F, of which there are six on each side of the tube, are for the purpose of receiving the trough and brass balls H, and on which half the weight of the tube is suspended, by the cross-beams and screw-bolts at G. This part of the apparatus does not, however, apply to the Britannia tower, but only to those at the edge of the water, and the land abutments, where the expansion and contraction take place. On the Britannia tower the tube is a fixture, and it advances and recedes from that point to the land on each side. Of the tube itself, an accurate view is here presented, showing its exterior and interior form. The cells at the top and bottom, as well as the railway platform, gussett-plates and other parts, are clearly shown, and I trust we may indulge the hope that the magnitude of the design and the nature of the structure will be clearly and explicitly understood.

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* See description of the sides, p. 189.

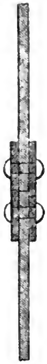
Plate VIII. fig. 1, is an elevation of one-half of one of the tubes of the Britannia Bridge. It consists of a vast number of plates, angle- and T-iron, riveted together, and exhibits the length, breadth, and thickness of each plate, as well as the general dimensions of the tube. The two first are represented by the lines on the drawings, and the thickness by the figures $\frac{10}{16}$, $\frac{9}{16}$, $\frac{8}{16}$, &c.

Figs. 2 and 3 exhibit the arrangement of the plates, which form the platforms of the upper and lower sides of the cells, on the top of the tube, and which in the working drawings are denominated the A and B platforms, as shown in the section at fig. 6.

Each of these platforms is composed of plates varying from $\frac{10}{16}$ to $\frac{12}{16}$ths of an inch in thickness. They contain eight plates in width; and to the edges of each row of plates, of both top and bottom platforms, are riveted the angle-irons of the X division plates, which also vary from $\frac{10}{16}$ to $\frac{12}{16}$ths of an inch in thickness; and from this connection we derive a perfectly strong and rigid cellular structure.

The transverse joints of these parts are carefully fitted against each other, with "butt" joints and double covering plates, riveted over each joint, as shown in the annexed sketch; and thus a platform, although composed of a number of pieces, is rendered homogeneous in its structure, and equally powerful in its resistance to compression, as if composed of one solid plate.

Fig. 58.



The X division plates, which separate the cells from each other, are riveted in the same manner, in their transverse joints, as the upper and lower platforms, and being firmly riveted with covering plates on both sides, a continuous rib of sheet-iron is formed, which, placed on edge, and riveted to the angle-iron, and the A and B platforms before described, forms a complete cellular construction on the top of the tube.

Figs. 4 and 5 represent the two bottom platforms D and E, which, having a connection with the V division plates, form a series of cells similar in character and appearance to those on the top of the tube.

The objects to be attained by the two cellular structures are, however, widely different; the one, as before observed, having to sustain a force tending to crush it, and the other a power, operating in an opposite direction, to tear it asunder. To resist these opposing forces, it will be observed, that the flexible nature of the material required a greater number of cells on the top than what appears to be necessary in the bottom. In fact, from the nature of the strain, cells are not required in the bottom; and it will be noticed, that in order to effect the greatest power of resistance, the plates are not only double the length of those on the top, but they are differently constructed, and differently jointed to those intended to resist compression. In the width of the D and E platforms, the plates are six in number; they are each 12 feet long, and are composed of two separate layers, one over the other, with alternate joints meeting on the middle of each plate; and by the use of long and strong covering plates "chain-riveted," as shown on Plate V., a powerful and continuous series of horizontal and vertical platforms are thus presented to the tensile strain of the tube. These platforms are composed of plates which vary in thickness from $\frac{9}{16}$ths in the middle to $\frac{7}{16}$ths at the ends, and are tongued or dovetailed into each other, as shown at A, A, A, &c., fig. 4. The breaks or jointings of the different thickness of the plates are represented in the same manner in all the other figures, each of which must be viewed as a separate and detached part of the tube.

The letters G, H, K, fig. 1, represent sections of those parts of the piers of the Britannia Bridge wheron the tubes rest. In that view K is the land abutment, H one of the piers at the margin of low water-mark, and G the Britannia tower, built on the rock of that name, in the middle of the straits.

Fig. 6 is a section of the tube, and to which the letters A, B, D and E, on figs. 2, 3, 4 and 5, refer.

PART III.

APPENDIX.

EXPERIMENTAL INQUIRY INTO THE STRENGTH OF MALLEABLE IRON TUBES.

EXPERIMENTAL INQUIRY

INTO THE

STRENGTH OF MALLEABLE IRON TUBES

FOR SUPPORTING

THE CHESTER AND HOLYHEAD RAILWAY

ACROSS

THE ESTUARY OF THE CONWAY, AND THE MENAI STRAITS.

THE preceding narrative already contains a statement of the objects for which the experiments herein recorded were instituted.

Considering the importance of the undertaking and the responsibilities which it involves, it will be necessary at the commencement to give a short description of the manner in which the experiments were conducted.

The peculiar nature of the investigation and the almost total absence of data, for the successful prosecution of the inquiry, operated in a great degree to retard its progress. The transverse strength of an iron tube composed of riveted plates, was an entirely new subject; and before the extent of its elasticity, rigidity, and the section of greatest strength were ascertained, it would have been extremely hazardous to have made use of such a tube for the support of a line of railway.

Previous to the commencement of the experiments, considerations of deep importance presented themselves. The extent of the span, the form of the tube, and the nature of the material of which it was to be composed were objects requiring the utmost attention. Each of these had to be considered separately, as well as in combination; and in searching for facts, with the view of discovering the law which governs the form and strength of the bridge, it was found necessary to proceed with caution, and to determine step by step the relative powers and resistances, which from time to time presented themselves during every successive experiment. These experiments were conducted with great care, and I trust, from the interest they have created and the novelty of their application, they will prove as interesting to the scientific inquirer, as they have been advantageous to the Company for whom they were made.

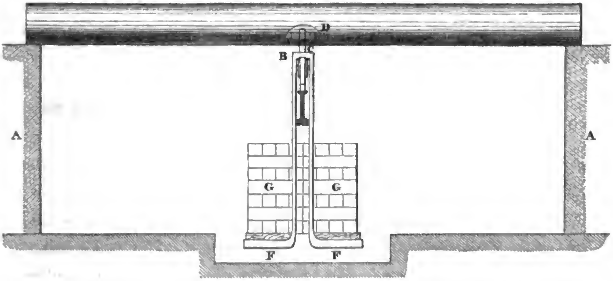
In conducting the present investigation, I should probably have shrunk from the responsibilities inseparable from such an undertaking, if it had not been for the encouragement I had received from Mr. Stephenson, as to my fitness for the task, and the assistance I anticipated from other quarters.

It is only known to those in search of physical truths, to what extent experiments must sometimes necessarily be carried, before results of importance can be fully elucidated and confirmed. New forms and new combinations are continually presenting themselves; and it not unfrequently happens that a well-conducted experiment, instead of rendering the subject more clear and explicit, only brings to light fresh anomalies and difficulties, and seems to open out an entirely new field of inquiry. Perhaps on no former occasion were experimental results more varied and perplexing than those presented during these researches; although generally favourable, they were not always in accordance with established theories, or such as the supposed nature of the experiments would indicate. On the contrary, weakness was found where strength was expected, and hence repeated changes of form, as well as changes in the distribution of the material, became absolutely necessary, not only to ensure comparative results, but to bring the two resisting forces of compression and extension in better agreement, or in perfect equilibrium with each other.

Taking the whole series of experiments, it was considered necessary to inquire into the powers of resistance offered,—first, by cylindrical tubes to a transverse strain; secondly, by those of the elliptical or egg-shaped form; and lastly, by those of a rectangular section, including such other modifications as the experiments might indicate during the progressive stages of the investigation.

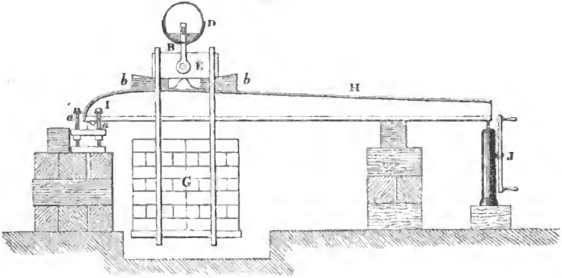
Each experiment is recorded, with some slight alteration, in the order in which it was made; but to give an accurate conception of the results, it will be necessary not only to describe the apparatus used in the course of the experiments, but also to show in what manner and under what circumstances those results were obtained.

Fig. 59.



The tube to be experimented upon was placed upon two solid blocks, A A (fig. 59), at a sufficient height to admit weights to be freely suspended from the tube. In the cylindrical and elliptical tubes, the weights were suspended from a hole cut through the middle of the lower sides. The pin or shackle B was supported upon a cotter and plate resting upon a cushion of hard wood, 8 inches square, fitted into the interior on the lower side of the tube. That part of the pipe, which was cut out to receive the iron pin, was strengthened by riveting a strong plate round the outside, as shown by the dotted lines at D. After adjusting the shackle, the cross plate E (fig. 60) was attached to the iron pin by a bolt, which fitted loosely into the forked ends of the pin B. To the cast-iron plate thus attached were suspended the large cross links F, and upon the forks of these links were laid the weights, on each side, as exhibited at G.

Fig. 60.



It will be observed, that by this arrangement the tubes were subjected to the test of a *dead* weight, and the complexity and uncertainty of a system of levers were thus avoided. A lever was however found necessary, for the purpose of raising the weights off the tube, in order to ascertain the defects of elasticity, or the amount of permanent deflection which had taken place when the load was removed. These powers of restoration as well as deflection, were carefully measured and recorded every time the weights were laid on, and also when they were subsequently removed. The large lever H, with the "screw-jack" J at the end, were used for raising and lowering the load, and for ascertaining the changes, which were going forward under the influence of the successive increase of the loading and unloading of the weights. The wooden wedges *b*, *b*, (fig. 60) were used, for the purpose of retaining the load steady during the time of raising the lever with the weights, in order to ascertain the permanent set. When the load was suspended, they were then removed.

It will be noticed, that in every experiment the deflections were taken after the weights were laid on; and as some of the tubes were deflected to a considerable extent, the fulcrum I, on which the large lever rested, was supported on an iron table, which had the power of being raised or lowered by the screws *a*, *a*, *a*, to suit the deflection of the tubes.

From this description it will be seen that every facility was afforded for marking the changes which took place during the progress of the experiments; and from the accuracy with which they were conducted, the following tables will be found to contain a faithful record of the derangement of the tubes, from the time of laying on the weights till their final destruction.

At the commencement of the experiments the thickness of the plates was measured by the Birmingham wire-gauge. That test was, however, found to be arbitrary and unsatisfactory, and it was therefore subsequently abandoned in favour of cutting the different plates into a number of small pieces; and having screwed them together in a vice, their collective thicknesses were taken, which divided by the number, gave, as nearly as possible, the thicknesses of the plates of which each part of the tube was composed.

The whole of the experiments had a direct reference to the proposed bridges across the Straits and the Conway river; and the diameters, lengths, depths, and thicknesses of each tube may be considered as certain proportional fractions of those structures. At the commencement the cylindrical tubes were simple approximations, and it was not until several of them were broken, that anything definite as to strength or form could be depended upon. It will be understood that the perfectly original character of the inquiry rendered the results extremely problematical, and having to search for facts, it was considered better and more desirable, in the first instance, to ascertain by direct experiment the form and proportions of each particular tube, than hazard the assumption of data which might afterwards prove erroneous and unsatisfactory.

The approximations were, however, of great value, as they exhibited defects and weakness which subsequently led to improved forms and conditions, both as regards the disposition of the material and the rigidity requisite to keep the tube in shape. All these requirements were carefully attended to; and the progress of the experiments gradually confirmed the fallacy of all preconceived opinions of the bearing powers and other properties of the material we were dealing with. Having however acquired considerable information, and attained a number of exceedingly interesting results, I found myself in a much better condition for laying before the mathematician such facts as would enable him to enter upon the theoretical investigation, and to deduce formulæ for the reduction of the experiments, and for the computation of the strength of every description of tubular bridges composed of sheet-iron riveted plates. Viewing the experiments as a scientific inquiry, and taking the results as deduced from them, it is easy to conceive what must be the strongest and best form of tube. Subsequent experiments may develope new properties and new combinations in the detail of construction, but I apprehend the great principle of form, or the section of greatest strength, will be found in the following tabulated results.

EXPERIMENTS ON THE TRANSVERSE STRENGTH OF MALLEABLE IRON TUBES OF THE CYLINDRICAL FORM.

Experiment I.—July 6th, 1845.

Cylindrical sheet-iron tube 18 feet 1 inch long, 12·18 exterior diameter, and 17 feet between the supports.

Thickness of 13 plates $=\cdot 53$ inch; $\therefore \frac{53}{13} = \cdot 0408$ inch, the thickness of the plates of which the tube was composed.

Weight of tube =102 lbs.

Weight of shackle =800.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse diameter.	Remarks.
800	·06			Before the whole of this weight was laid on, the tube failed from compression by puckering on the upper side.
1920	·25			
3040	......	......	......	Crushed by compression.
∴ Ultimate deflection = ·39.				See Plate X. fig. 1.

Experiment I. repeated.—October 9th, 1845.

This experiment was repeated on one of the fractured halves of the same tube, 8 feet 6 inches between the supports.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse diameter.	Remarks.
960	·02	......	12·010	
2695	·13	......	11·912	
3970	......	......	......	Crushed on the top sides 6 inches from the centre of the tube.
∴ Ultimate deflection = ·19.				Plate X. fig. 1.

In the above experiments the tubes failed successively by compression; the top doubling up close to the first row of rivets 13 inches from the shackle. The half-length tube also crushed on the top side, 6 inches from the centre, after sustaining the weight, 3970 lbs., for some minutes.

Experiment II.—July 7th, 1845.

Cylindrical sheet-iron tube 18 feet 1½ inch long, 12 inches exterior diameter, and 17 feet between the supports.

Thickness of 15 plates $= \cdot 56$ inch; $\therefore \frac{56}{15} = \cdot 037$ inch, the thickness of the plates.

Weight of tube $= 107$ lbs.

Weight of shackle $= 800$ lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse diameter.	Remarks.
800	·20	......	12·00	
1360	·32	·03	12·00	
1920	·41	·03	11·50	
2114	·46	·03	11·40	
2256	·60	·05	11·25	
2368	·60	·10	11·15	
2480	·61	·10	11·10	
2592	·61	......	11·00	
2704	·65	......	......	Crushed on the top side about 10 inches from the shackle after sustaining the load about 1½ minute.
∴ Ultimate deflection = ·65.				Plate X. fig. 2.

From the exceedingly flexible nature of the material, the tube in this experiment failed, as before, by compression, the upper side collapsing at a distance of 10 inches from the shackle.

Experiment III.—July 11th, 1845.

Cylindrical sheet-iron tube 16 feet 10 inches long, 12·4 inches exterior diameter, and 15 feet 7½ inches between the supports.

Thickness of 9 plates $= 1 \cdot 18$ inch; $\therefore \frac{1 \cdot 18}{9} = \cdot 113$ inch, the thickness of the plates.

Weight of tube $= 392$ lbs.

Weight of shackle $= 800$ lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse diameter.	Remarks.
800	·07	……	12·40	
1,920	·17	……	12·32	Up to the load when 6960 lbs. was laid on, the tube apparently sustained no injury in its transverse diameter.
3,040	·30	·025	12·25	
3,600	·31	·025	12·25	
4,160	·35	·050	12·20	
4,720	·42	·075	12·19	
5,280	·45	·076	12·19	
5,840	·50	·100	12·17	
6,400	·56	·102	12·15	
6,960	·65	·102	12·15	
7,520	·72	·150	12·15	With the weight 8080 lbs. the sides were slightly collapsed, taking the form of the ellipse.
8,080	·85	·175	12·17	
8,640	·87	·225	12·10	
9,200	·97	·250	12·07	
9,760	1·02	·300	12·07	
10,320	1·12	·350	12·04	
10,880	1·24	·400	12·00	
11,440	……	……	……	Broke with this weight, the lower part tearing asunder across the shackle hole.
∴ Ultimate deflection = 1·29.				Plate X. fig. 3.

From this experiment it appears that the elastic powers of sheet-iron cylindrical tubes are but slightly injured by a strain of 8640 lbs. (upwards of three-fourths the breaking weight) with a deflection of ·87 inch, and exhibits only ·225 inch as the defect of elasticity.

Experiment IV.—July 30th, 1845.

Cylindrical sheet-iron tube 24 feet $10\frac{1}{2}$ inches long, 18·26 inches exterior diameter, and 23 feet 5 inches between the supports.

Thickness of 17 plates $= \cdot 99$ inches; $\therefore \frac{\cdot 99}{17} = \cdot 0582$ inch, the thickness of the plates.

Weight of tube = 334 lbs.

Weight of scale and shackle = 800 lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse diameter.	Remarks.
800	·08	……	17·75	
1920	·15	……	17·75	From the flexible nature of the tube and the thickness of the plates the permanent deflection could not be ascertained.
3040	·25	……	17·48	
4160	·35	……	17·15	
5280	·46	……	16·65	
5840	·51	……	16·63	
6400	……	……	……	Broke 6 inches from the centre of the shackle, the bottom side tearing close to the edge of the plate riveted round the shackle hole.
∴ Ultimate deflection = ·56.				Plate X. fig. 4.

In this experiment the upper side of the tube resisted the force of compression with greater tenacity than those in the previous experiments, arising probably from the comparatively small amount of deflection and from stiffness of the plates.

Experiment V.—July 12th, 1845.

Cylindrical sheet-iron tube 25 feet long, 17·68 inches exterior diameter, and 23 feet 5 inches between the supports.

Thickness of 16 plates $=1{\cdot}01$ inch; $\therefore \frac{1{\cdot}01}{16}={\cdot}0631$ inch, the thickness of the plates.

Weight of tube $=346$ lbs.

Weight of scale and shackle $=800$ lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse diameter.	Remarks.
800	·10	......	17·98	
1920	·20	......	17·98	
3040	·30	......	17·58	Sides slightly changed in form, load removed.
4160	·42	·050	17·28	
5280	·51	·075	16·98	
5840	·60	·090	16·65	
6120	·71	·100	16·48	
6400	......	......	......	Broke on the lower side 25 inches from the centre, the plate tearing through the rivet-holes, which at that point were much weakened.
∴ Ultimate deflection = ·74				Plate X. fig. 5.

With the heavier weights it required the utmost care to retain the ends of the tube, which rested upon the blocks, in shape. All of them were protected by a circular block of wood fitted into each end, in order to prevent distortion and crushing at that part.

Some weeks after the foregoing experiment was made, it was deemed advisable to repeat the experiment upon the longest uninjured part of the tube. This was done to determine the relative values of the two resisting forces of compression and extension, in a tube whose plates were not so much weakened by the rivets, as those in the last and some of the previous experiments. The results are as follow:—

Experiment V. repeated on one of the fractured halves, October 8th, 1845.

Cylindrical sheet-iron tube 14 feet 8 inches long, 17·68 inches exterior diameter, and 14 feet between the supports.

Thickness of plates as before.

Weight of tube =215 lbs.

Weight of scale and shackle =960 lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse diameter.	Remarks.
960	......	......	17·62	
1,823	·06	......	17·57	
2,693	·10	......	17·32	From the shortness of the tube the top side instead of being deflected was raised as the weights were laid on. This arose from the distortion of the sides in the middle, which were elongated; 11050 lbs. were left on the tube for some minutes.
3,570	·14	......	17·14	
4,437	·20	......	17·02	
5,300	·25	......	16·80	
6,157	·30	......	16·68	
7,021	·32	......	16·38	
7,854	·38	......	16·27	
8,272	·42	......	16·07	
8,695	·50	·16	15·98	
9,126	·56	·25	15·52	
9,560	·58	·28	15·27	
9,976	·60	·40	15·02	
10,395	·62	·47	14·67	
10,826	·92	·60	14·02	
11,050	·92	·93	13·92	
11,274	......	......	......	Destroyed by compression, the top side puckering from the collapse of the sides, which were greatly distorted.
∴ Ultimate deflection =·958.				Plate X. fig. 5.

The experiments on this tube indicated results somewhat different to those previously experimented upon. The ratio of the diameter to the distance between the supports, and the flexible nature of the tube, caused the centre of the tube, where the strain was applied, to collapse and take the elliptical form. This property is strongly marked in the column of deflections, where the top side rises, instead of following the bottom, as in the other experiments.

Experiment VI.—July 12th, 1845.

Cylindrical sheet-iron tube 25 feet 1 inch long, 18·18 inches exterior diameter, and 23 feet 5 inches between the supports.

Thickness of 10 plates =1·19 inch; $\therefore \frac{1\cdot19}{10} = \cdot119$ inch, the thickness of the plates.

Weight of tube =777 lbs.

Weight of scale and shackle =800 lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse diameter.	Remarks.
800	·05	......	18·00	
1,920	·15	......	18·00	
3,040	·20	......	17·95	
4,160	·30	......	17·90	
5,280	·38	·05	17·90	
6,400	·45	·06	17·90	
7,520	·55	·06	17·88	
8,649	·64	·07	17·83	
9,760	·75	·09	17·75	
10,180	·84	·10	17·70	
12,000	·95	·13	17·65	
12,560	1·00	·15	17·64	
13.120	1·05	·18	17·60	
13,680	1·13	·20	17·58	
14,240	......	......	......	Broke through the rivet-holes, 3 feet 2½ inches from the centre, after sustaining the weight for half a minute.
∴ Ultimate deflection = 1·19				Plate X. fig. 6.

It will be observed that nearly the whole of the tubes that failed from extension gave way at the rivet-holes, or in the immediate neighbourhood of the shackle where the plates were weakened. Had those parts been of the same strength as the plates, it is probable that the whole, or nearly the whole of them, would have yielded to compression.

Another experiment was made on this tube at 15 feet between the supports, as follows:—

Experiment VI. repeated on one of the fractured halves, October 8th, 1845.

Cylindrical sheet-iron tube 25 feet 1 inch long, 18·18 inches diameter, and 15 feet between the supports.

Weight of tube =475 lbs.

Weight of scale and shackle =930 lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse diameter.	Remarks.
930	+	......	18·30	
2,687	·04	......	18·30	
4,393	·06	......	18·30	
6,150	·06	......	18·20	
7,853	·07	......	18·17	
9,541	·07	......	18·10	
11,243	·08	......	18·07	
12,974	·08	......	18·03	
14,682	·08	......	17·90	
16,425	·08	......	17·89	
18,111	......	......	......	This experiment was lost from the foundations giving way, it was however resumed with a weight of 16837 lbs.
16,837	......	......	17·77	
18,555	......	......	......	Broke through the rivets 8 inches from the centre on the lower side.
∴ Ultimate deflection =·114.				Plate X. fig. 6.

During the whole of these experiments considerable changes of form were observable, by the elongation of the vertical diameter at the shackle, and the extension of the transverse diameter at the ends resting upon the supports.

These changes were more strikingly apparent in cases where the circular blocks were not well fitted into the ends of the tube.

Experiment VII.—July 30th, 1845.

Cylindrical sheet-iron tube 32 feet 8 inches long, 2 feet exterior diameter, and 31 feet $3\frac{1}{4}$ inches between the supports.

Thickness of 11 plates $= 1\cdot05$ inch; $\therefore \frac{1\cdot05}{11} = \cdot0954$ inch, the thickness of the plates.

Weight of tube =1007 lbs.

Weight of scale and shackle =800 lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse diameter.	Remarks.
800	·04	......	23·90	From the commencement of laying on the weights a change is produced in the shape of the tube, but no perceptible set before 4160 lbs. was laid on.
1920	·10	......	23·85	
3040	·17	......	23·72	
4160	·25	·02	23·54	
5280	·33	·03	23·36	
6400	·40	·04	23·30	
7520	·47	·05	23·14	
8640	·55	·05	23·06	
9760	......	......	......	Broke on the underside by tearing along the line of the rivets as before, 13½ inches from the middle of the tube.
∵ Ultimate deflection = ·63.				Plate XI. fig. 7.

The chief defect in this experiment, as well as in some others here recorded, is the weakness of the plates at the riveted joints.

Experiment VIII.—July 30th, 1845.

Cylindrical sheet-iron tube 34 feet 6½ inches long, 24·3 inches exterior diameter, and 31 feet 3¼ inches between the supports.

Thickness of 11 plates = 1·03 inches; $\therefore \frac{1{\cdot}19}{10} = {\cdot}119$ inch, the thickness of the plates.

Weight of tube = 1385 lbs.

Weight of scale and shackle = 800 lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse diameter.	Remarks.
800	·05	......	24·10	No perceptible changes in the defects of elasticity up to the load 4160 lbs.
1,920	·15	......	24·05	
3,040	·20	......	24·00	
4,160	·27	·03	23·86	
5,280	·33	·04	23·80	
6,400	·40	·05	23·75	
7,520	·48	·07	23·72	
8,640	·52	·08	23·70	
9,760	·60	·10	23·67	
10,880	·69	·11	23·60	The sides or horizontal axes at this point have taken a permanent set.
12,000	·77	·13	23·55	
12,560	·81	·14	23·50	
13,120	·85	·15	23·42	
13,680	·90	·17	23·40	
14,240	......	......	......	Broke through the rivets 15 inches from the centre as the weight was suspended on the tube.
∴ Ultimate deflection = ·93.				Plate XI. fig. 8.

The same defects, arising from weakness at the riveted joints, is here observable as on former occasions. All the tubes hitherto experimented upon have been more or less defective in this respect, and hence arises the absolute necessity of exercising the utmost care in fixing upon a sound principle upon which joints of this description should be constructed.

Experiment IX.—July 31st, 1845.

Cylindrical sheet-iron tube 32 feet 8¼ inches long, 24·2 inches exterior diameter, and 31 feet 3¼ inches between the supports.

Thickness of 11 plates $=1\cdot08$ inch; $\therefore \frac{1\cdot08}{11}=\cdot098$ inch, the thickness of the plates.

Weight of tube $=1005$ lbs.

Weight of scale and shackle $=800$ lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse diameter.	Remarks.
800	+	......	23·76	
1,920	·05	+	23·76	
3,040	·13	+	23·60	Up to the weight 5280 lbs. no change had taken place in the transverse axis.
4,160	·21	+	23·50	
5,280	·30	·01	23·42	
6,400	·39	·03	23·36	
7,520	·47	·04	23·30	
8,640	·52	·06	23·22	
9,200	·59	·07	23·16	
9,760	·64	·08	23·10	
10,320	·69	·10	23·05	
10,880	......	......	......	Broke as before through the rivet holes, 15 inches from the shackle.
∴ Ultimate deflection = ·72.				Plate XI. fig. 9.

In this experiment the same indications of weakness were apparent as in all those previously tested. The form is evidently imperfect, and exhibits great weakness when subjected to a transverse strain. The collapsing of the sides, and the excess of material which surrounds the neutral axis, are much against this form of tube. Its strength would however be greatly improved by a better system of riveting, and by increasing the thickness of the plates on the upper and lower sides.

The principal objection to this kind of tube is the difficulty which exists in retaining the tube in its form. Under severe strain the sections are no longer circles but ellipses; in the middle section the vertical diameter is increased in length, and, on the contrary, at each end it is decreased. These changes of form give an appearance of distortion to the tube, weaken its powers of resistance, and render it unfit to sustain the load.

EXPERIMENTS ON THE TRANSVERSE STRENGTH OF MALLEABLE IRON ELLIPTICAL TUBES.

Experiment XIX.—August 6th, 1845.

Elliptical sheet-iron tube 17 feet 11 inches long, vertical axis 14·62 inches, transverse axis 9·25 inches, and 17 feet between the supports.

Thickness of 26 plates $=1·08$ inch; $\therefore \frac{1·08}{26} = ·0416$ inch, the thickness of the plates.

Weight of tube $=109$ lbs.

Weight of shackle $=800$ lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse axis.	Permanent set in transverse axis.	Remarks.
800	·12	·02	14·62		
1920	·57	·09	14·30		
2100	……	……	……	……	Crushed on the top side just as the whole weight was suspended.
∴ Ultimate deflection ≐ ·607.					Plate XII. fig. 15.

Here the breaking weight is considerably less than that which is recorded in Experiment I. on a cylindrical tube, of nearly the same weight and thickness of plates.

Experiment XX.—September 17th, 1845.

Elliptical sheet-iron tube 25 feet 9 inches long, vertical axis 21·66 inches, transverse axis 13·5 inches, and 24 feet between the supports.

Thickness of 9 plates $=1·18$ inch; $\therefore \frac{1·18}{9} = ·1310$ inch, the thickness of the plates.

Weight of tube $=708$ lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse axis.	Permanent set in transverse axis.	Remarks.
848	·02	……	14·00		
1,744	·08	……	14·00		
2,640	·13	……	14·00		
3,536	·17	……	13·99	……	With the weight 16,528 lbs. the scales lost their balance and the weights fell off. They were subsequently replaced, and the same weights again laid on. The deflection when taken indicated 1·32, as shown in the table.
4,432	·22	……	13·85		
5,328	·28	·020	13·79		
6,224	·34	·025	13·67		
7,120	·39	·040	13·61		
8,016	·45	·045	13·60		
8,912	·51	·060	13·55		
9,808	·58	·080	13·46		
10,704	·64	·100	13·40		
11,400	·71	·125	13·36		
12,496	·80	·154	13·25	·05	
13,392	·90	·200	13·20	·10	
14,288	1·00	·240	13·15	·15	
14,736	1·05	·275	13·10	·24	
15,184	1·13	·300	13·04	·26	
15,632	1·20	·360	12·96	·31	
16,080	1·27	·390	12·94	·37	
16,528	1·32	·440	12·82	·38	
17,076	……	……	……	……	Broke after sustaining the load for a few seconds, the lower side tearing asunder through the shackle hole.
∴ Ultimate deflection = 1·35.					Plate XIII. fig. 19.

From the commencement of the experiment to its termination, the tube gave indications of great stiffness and strength. The deflections and defects of elasticity were progressive throughout the whole series, and in this experiment, when the last weight 17,076 lbs. was laid on, a slight crackling noise was heard previous to rupture.

Experiment XXI.—September 18th, 1845.

Elliptical sheet-iron tube 26 feet 3 inches long, vertical axis 21·25 inches, transverse axis 14·12 inches, and 24 feet between the supports.

Thickness of 17 plates $=1 \cdot 17$ inch; $\therefore \frac{1 \cdot 17}{17} = \cdot 0688$ inch, the thickness of the plate.

Weight of tube $=357$ lbs.

Weight of shackle $=848$ lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse axis.	Permanent set in transverse axis.	Remarks.
848	·04	......	13·62		
1731	·09	......	13·26		
2580	·13	......	13·12		
3436	·17	......	12·77	·05	
4289	·22	......	12·57	·06	
5146	·28	......	12·32	·10	
5986	·34	......	12·02	·20	
6834	·40	......	11·72	·25	
7714	......	......	......	......	Broke with laying on the weight, the top side doubling up. Probably one-half the weight laid on, or 7270 lbs. might have caused fracture.
∴ Ultimate deflection = ·45.					Plate XIII. fig. 20.

Looking at the effects produced by the heavier weights upon this tube, and comparing it with one previously experimented upon, it is evident that stiffness is an important element in this description of pipe, in order to prevent the sides collapsing when the weights are applied. This is strongly exemplified in the 4th column of the table, where the changes of the transverse axis vary from 13·62 to 11·72, making a difference of nearly 2 inches when suffering under the influence of severe pressure.

Experiment XXII.—September 18th, 1845.

Elliptical sheet-iron tube, with a cell on the top side, 19 feet 8 inches long, vertical axis 12 inches, transverse axis 7½ inches, and 18 feet 6 inches between the supports.

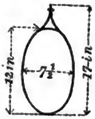
Thickness of 16 plates = 1·17 inches; $\therefore \frac{1\cdot17}{16} = \cdot0733$ inch, the thickness of plates.

Weight of tube = 232 lbs.

Weight of shackle = 848 lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Changes in transverse axis.	Permanent set in transverse axis.	Remarks.
848	·125	......	7·50	......	During the early part of the experiment the alterations in shape were steadily progressive, or until 6016 lbs. were laid on, when they became more anomalous, which is indicated by the restoration of the transverse axis with the 6439 weights.
1728	·200	......	7·46		
2601	·300	......	7·45		
3469	·400	......	7·44		
4324	·510	+	7·35		
5178	·625	·04	7·33		
5598	·700	·08	7·32		
6016	·780	·10	7·44		
6439	·870	·14	7·45		
6867	......	......	......	......	With this weight the top edge or fin part doubled up, still showing weakness on the top side.
Experiment repeated after reversing the tube with fin downwards.					
930	·14	+	7·50		
2641	·61	·14	7·48		
4398	1·19	·42	7·15	......	For some time previous to the bottom tearing asunder, the upper side was out of shape.
4822	1·32	·58	7·13		
5237	1·45	·67	7·13		
5648	......	......	......	......	Broke by extension 22 inches from the middle, after sustaining the weight a few seconds.
∴ Ultimate deflection = ·892.					Plate XIV. fig. 21.

Fig. 61.

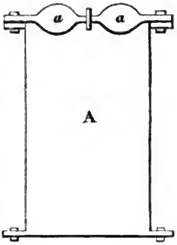


As the whole of the experiments on the elliptical tubes gave evident signs of weakness to a crushing force, it was deemed advisable to strengthen the tube, by riveting a cellular fin (such as is shown in the annexed sketch) along the top side of the tube. This addition did not, however, answer the intended purpose, as will be seen by its yielding to compression when the last weight 6867 lbs. was laid on.

From this it would appear, that the resisting powers to a crushing force on the top side are still unequal to the pressure in that direction; and viewing the experiments upon the elliptical tubes, as far as they have gone, they may be considered of an inferior description, and undoubtedly imperfect as regards the best form for sustaining a heavy transverse strain.

From the last experiment it is obvious, that the two resisting powers of extension and compression are not equalized, and in order to produce fracture by a tensile strain, it will be necessary to enlarge and increase the quantity of material on the top side, and also to give it greater rigidity than has yet been attained in that part. For this object, it appears desirable to repeat the experiment with one or more tubes, constructed of the rectangular form with a corrugated top, as per annexed sketch at A, and to have them riveted together in the form of two cells, as shown at *a*, *a*.

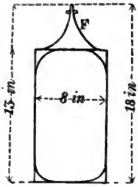
Fig. 62.



These corrugations would add considerably to the strength of the upper side of the tube, and tend to equalise the two opposing forces of resistance to extension and compression. This form was subsequently adopted*, and found exceedingly satisfactory as regards strength and its powers of resistance under severe strain.

Finding the same difficulty in retaining the elliptical tubes in shape that we found in the cylindrical ones, the form was changed from the ellipse to a tube with straight sides and curvilinear top and bottom, with the fin repeated on the top side, as at F, to resist crushing in that part.

Fig. 63.



The results of the experiments on this tube are as follow :—

---

* Whilst engaged in making the experiments, I had frequently occasion to take notes and make remarks upon the powers of resistance, &c. which each successive form of tube offered to the strain acting upon it. As many of these notes are interesting, and as the results recorded were such as to suggest new forms and new combinations, I shall on this, as on other occasions, take the liberty of introducing them nearly in the same form and in the same order as they were made at the time. In this treatment of the subject there may be some slight irregularity, but the importance of the inquiry, and the freshness of the ideas as they presented themselves at the moment, must plead my excuse.

Experiment XXIII.—September 19th, 1845.

Rectangular sheet-iron tube, with single cell on the top, and circular top and bottom, 19 feet 1 inch long, depth 13 inches, width 8 inches, and 18 feet 6 inches between the supports.

Thickness of 17 plates $=1{\cdot}095$ inch; $\therefore \frac{1{\cdot}095}{17}={\cdot}064$ inch, the thickness of the plates.

Weight of tube =267 lbs.

Weight of shackle =848 lbs.

Weight in lbs.	Deflection in inches.	Deflection in inches, load removed.	Remarks.
848	·080	……	After 7136 lbs. were laid on, the top fin was evidently much destroyed.
1722	·162		
2518	·241	……	The two following weights did not, however, make much change; the upper side of the tube, having come into action, retained it in shape until the whole gave way.
3469	·324		
4341	·413		
5179	·516		
6021	·604	·025	
6867	·723	·034	
7316	·798	·050	
7540	·830	·057	
7652	·852	·061	
7876	·900	·072	
8100	·974	·082	
8406	1·140	·260	
8812	……	……	With this weight the top side doubled up 3 feet from the centre of the tube.
∴ Ultimate deflection = 1·140.			Plate XIV. fig. 22.

The difference between the strength of the elliptical tube with a fin, and the rectangular form having a similar top, is very inconsiderable. They are nearly of proportionate weights, and having the same distance between the supports, the comparison is nearly perfect; the first crushing on the top side with a force of 6867 lbs., and the other distorted in the same manner by a force of 8812 lbs.

It is obvious from the two last experiments, that in adding to the strength of the top side great care should be observed in retaining it perfectly flat, in order to concentrate the material at as great a distance as possible from the neutral axis; and the same rule will apply to the bottom. It therefore follows that all girders, be they hollow or otherwise, should on no account terminate in an apex, as represented in the two last cases before us.

Experiment XXIV.—September 19th, 1845.

Elliptical sheet-iron tube 18 feet 2½ inches long, vertical axis 15 inches, transverse axis 9¾ inches, and 17 feet 6 inches between the supports.

Thickness of 9 plates $=1\cdot29$ inch; $\therefore \frac{1\cdot29}{9}=\cdot143$ inch, the thickness of the plates.

Weight of tube $=374$ lbs.

Weight of shackle $=848$ lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Changes in transverse axis.	Transverse axis.	Remarks.
848	·05	......	7·62	......	15,000 lbs. would probably have produced fracture, as the tube was suffering from the crushing power on the top at the same instant the bottom was tearing asunder.
2,137	·14	......	7·62		
3,448	·22	......	7·62		
4,737	·31	·040	7·58		
6,012	·40	·045	7·54		
7,327	·49	·057	7·52		
8,580	·58	·070	7·51		
9,870	·69	·110	7·50		
11,180	·82	·158	7·49		
12,461	·96	·240	7·48		
13,755	1·16	·340	7·42		
14,619	1·36	·470	7·40		
15,490	......	......	......	......	Broke just as the whole of the weight was laid on.
∴ Ultimate deflection = 1·45.					Plate XIV. fig. 23.

The foregoing completes the experiments on elliptical tubes, and although the elliptical is inferior in strength to the rectangular form, the results are nevertheless highly instructive. It will be observed, that in almost every case where the tubes had cells on the top side, the two sustaining forces approached nearer to each other than in those of the cylindrical or rectangular form where the plates were of uniform thickness. In these experiments, as well as in all the others, the top side is almost invariably the weakest.

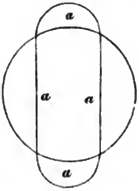
Observations on the foregoing Experiments.

Every horizontal beam, subjected to a transverse strain, has two important offices to perform: first the resistance it offers to compression on its upper side, and secondly the power which it opposes to tension on the lower side. Now it is immaterial whether the beam be hollow or solid; its elements or characteristics of strength are the same; and all properly constructed beams must have that form which presents the greatest resistance to compression on the one hand, and to tension on the other. The further that the molecules, or particles of which a beam is composed, are removed from the neutral axis, the greater the powers of resistance to sustain the load. In order, therefore, that a beam should have the strongest section with a given quantity of material, the forces of compression and tension must not only be duly balanced, but the material should be accumulated at the top and bottom of the section, where the strain is the greatest; that in the sides serving only to maintain the connection between them. Now the cylindrical form, having such a great proportion of the material in the vicinity of the neutral axis, is certainly not well-calculated for resisting a transverse strain. The same objections apply, though in a less degree, to the elliptical tubes.

On consulting the tables, it will be found, that nine distinct experiments were made upon cylindrical tubes. Out of this number seven were torn asunder through the rivet-holes, and two yielded to compression. It is more than probable, that the whole of them would have puckered on the top side, had the riveting been sound. Unfortunately that was not the case, and hence followed the rupture of the bottom through the line of the rivet-holes, which from their number and closeness had greatly injured the strength of the plates. One remarkable feature of these experiments, however, was the distortions which the whole of the tubes presented under severe strain. With less than half the breaking weight the sides of the tubes were collapsed, and before three-fourths of the load were suspended, the middle section presented an elliptical form, which gradually increased with the load until rupture took place.

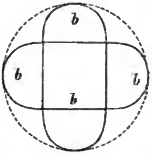
These contortions were strikingly apparent in the tubes composed of thin plates, and in some of the experiments the sides became so much collapsed as to assume almost a straight line on both sides, in the middle, with the top rising and the bottom descending in the form of the annexed figure at *a*, *a*, *a*, *a*. This change of figure rendered it next to impossible to ascertain the deflection, which rapidly increased in both directions, and magnified the distortions to such a degree as to render the tube totally inapplicable for the purpose intended.

Fig. 64.



Another discrepancy was the insecurity of the ends resting upon the supports which greatly increased the disfigurement of the tube, unless retained in shape by a circular piece of wood closely fitted into each end. The removal of these blocks, when the tube was under strain, presented a singular distorted appearance: both ends became flattened horizontally, and the middle elongated in a vertical direction so as to present two elliptical sections, as shown in the diagram, which is here exaggerated as at *b*, *b*, *b*, *b*, in order to exhibit the insufficiency of cylindrical tubes to support a severe transverse strain. Some of the stronger description of tubes when composed of thicker plates gave indications of increased strength; but not more than might reasonably have been expected from the increased thickness and greater rigidity of the material.

Fig. 65.



Taking the whole of the experiments on the circular tubes, it will appear obvious from the pressure to which all the parts are subjected, that they are not calculated to resist a heavy transverse strain. Altogether the form and distribution of the material are defective, and cannot therefore be trusted for such a purpose. Reasoning from these facts, and taking the whole of the circumstances indicated by the experiments into consideration, I have no hesitation in pronouncing the circular form of tube exceedingly defective, and not to be relied on as a medium of support for railway traffic across the Menai Straits.

Summary of Results obtained from the Experiments on Cylindrical Tubes.

Distance between supports.		Diameter in inches.	Thickness of plate in inches.	Ultimate deflection in inches.	Breaking weight in tons.	Remarks.
ft.	in.					
17	0	12·18	·0408	·39	3,040	Crushed top.
17	0	12·00	·0370	·65	2,704	Crushed top.
15	7½	12·40	·1310	1·29	11,440	Torn asunder at bottom.
23	5	18·26	·0582	·56	6,400	Torn asunder at bottom.
23	5	17·68	·0631	·74	6,400	Torn asunder at bottom.
23	5	18·18	·1190	1·19	14,240	Torn asunder at bottom.
31	3¼	24·00	·0954	·63	9,760	Torn asunder at bottom.
31	3¼	24·30	·13501	·95	14,240	Torn asunder at bottom.
31	3¼	24·20	·0954	·74	10,880	Torn asunder at bottom.

Summary of Results from the Experiments on Elliptical Tubes.

No. of experiments.	Distance between the supports.		Diameter congregate and transverse.	Thickness of plates.	Ultimate deflection in inches.	Breaking weight in lbs.	Remarks.
	ft.	in.					
19	17	0	14·62 9·25	·0416	·62	2,100	Crushed on top.
20	24	0	21·66 13·50	1·1320	1·36	17,076	Broke by extension.
21	24	0	21·25 14·12	·0688	·45	7,270	By compression.
22	18	6	12·00 7·50	·0775	·95	6,867	By compression; this tube had a fin on the top side.
24	17	6	15·00 9·75	·1430	1·39	15,000	Both sides were ruptured.

It will be observed that the whole of the experiments on the elliptical tubes indicated weakness in the top side, which, in almost every case, was greatly distorted by the force of compression acting on that part. The anomalous results of the top side yielding to compression, suggested a modified form of tube, constructed by riveting a hollow longitudinal fin along its upper side. This was done in the manner described in Experiments XVI. and XVII., but without effect, as the top side was still found defective, and gave way in both experiments by the doubling up of that part. The elliptical tubes also exhibited considerable distortions in form some time before the load producing rupture was laid on.

On consulting the preceding experiment, it will be observed, that nearly the whole of the tubes are comparatively weak, arising in a great measure from their defective form, and partly from the injury done to the plates by an injudicious system of riveting which the workmen had introduced. These defects were remedied in the subsequent experiments on the rectangular tubes, and great care was taken to make the joints as nearly as possible of the same strength as the plates.

The next series of experiments are not only highly interesting in themselves, but involve practical considerations of deep importance in relation to the future interests of civilised society. These experiments show, that the rectangular form of the tubular wrought iron girder is much better calculated to resist transverse strain than any other form that can be adopted, provided the sectional parts are so arranged and distributed, as to give the greatest strength with the least quantity of material.

EXPERIMENTS ON THE TRANSVERSE STRENGTH OF MALLEABLE IRON RECTANGULAR TUBES.

Experiment XIV.—July 31st, 1845.

Rectangular tube 18 feet 6 inches long, 9·6 inches square, and 17 feet 6 inches between the supports.

Thickness of plates, top . . . . $\frac{1\cdot05}{14} = \cdot075$ inch.
,, ,, bottom . . $\frac{1\cdot04}{14} = \cdot0743$,,
,, ,, sides . . . . $\frac{1\cdot04}{14} = \cdot0743$,,
Weight of tube = 202 lbs.
Weight of shackle = 938 lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
938	·17		
2058	·55		
3178	·96		
3738	......	......	Yielded to compression, the top side doubling up, and the sides bulging close to the injured part.
∴ Ultimate deflection = 1·12.			Plate XII. fig. 10.

The great weakness indicated by this tube in yielding to a weight of only 3738 lbs., caused a different arrangement of the plates, a new one of nearly three times the thickness was prepared and riveted on the top side. The plate on the lower side was also strengthened at the joints, and having repaired the other parts injured, the tube was again put to the test as follows:—

Experiment XIV. *a* repeated.—October 9th, 1845.

Rectangular tube 18 feet 6 inches long, 9·6 inches square, and 17 feet 6 inches between the supports.

Thickness of plates, top . . . . $\frac{1·26}{5} = ·252$ inch.
„ „ bottom . . $\frac{1·05}{14} = ·075$ „
„ „ sides . . . . $\frac{1·04}{14} = ·074$ „
Weight of tube = 384 lbs.
Weight of shackle = 960 lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
960	·07		
1854	·16		
2717	·25		
3568	·35		
4441	·45	·07	
5310	·55	·10	
6180	·67	·11	
6180	·73	......	This weight was repeated in consequence of the tube being slightly distorted.
7017	·84	·14	
7427	·94	·20	
7859	1·07	·29	
8273	......	......	Broke asunder by tearing at a joint on the bottom side, 11 inches from the shackle, where the plate was weakened.
∴ Ultimate deflection = 1·10.			Plate XII. fig. 10.

Here, by increasing the material at the top of the tube, we attained more than double the strength; thereby showing that fibrous bodies, like wrought-iron, being more ductile, are more susceptible of injury from compression than from extension. This law is further confirmed by the succeeding experiments.

Experiment XV.—July 31st, 1845.

Rectangular tube 18 feet 6 inches long, 9·6 inches square, and 17 feet 6 inches between the supports.

Thickness of plates, top . . . $\frac{1\cdot06}{14}$ = ·0757 inch.
" " bottom . . $\frac{1\cdot14}{8}$ = ·142 "
" " sides . . . . . = ·0757 "

Weight of tube = 255 lbs.

Weight of shackle = 988 lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
988	·16	......	In this experiment great weakness is exhibited as well as in the former one.
2108	·45	·05	
3228	·80	·09	
3788	......	......	With this weight, the top plate began to buckle 2 feet 6 inches from the shackle on one side, and 6 inches from it on the other. It appears to require stiffness in order to resist the tendency to pucker.
∴ Ultimate deflection = ·94.			Plate XII. fig. 11.

This experiment was repeated with a strong plate 2 feet 7 inches long, 11 inches wide, and 1·1 inch thick, laid along the top, in order to stiffen it and throw the strain more upon the bottom plate. The results were however unimportant, until the tube was reversed with the thick side upwards, when a very important change was effected, as shown in the succeeding experiment.

Experiment XV. *a*.—July 31st, 1845.

Rectangular tube, the same as before. Tube reversed, with the thick side uppermost.

Thickness of plates, top . . . $\frac{1\cdot14}{8}$ = ·142 inch.
" " bottom . . $\frac{1\cdot06}{14}$ = ·0757 "
" " sides . . . $\frac{1\cdot06}{14}$ = ·0757 "

Weight of tube = 255 lbs.

Weight of shackle = 988 lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
988	·17	......	The deflection as well as the modulus of elasticity are much greater in this than in any of the former experiments.
2108	·50	·07	
3228	·78	·14	
3788	·90	·18	
4348	1·05	·20	
4908	1·21	·26	
5468	1·37	·32	
6028	1·54	·40	
6588	1·75	·50	
7148			
∴ Ultimate deflection = 1·76.			Plate XII. fig. 11.

If we compare the results of the two last experiments with those contained in Experiments XIV. and XIV. *a*, we shall find that the proportions of the top and sides are widely different. In both cases, however, when the tube was reversed with the thick side uppermost, double, or nearly double, the strength is obtained. Hence it follows, that in order to obtain the section of greatest strength, the top side of a tube, when submitted to a transverse strain, must be considerably thicker than its lower side. This fact is fully established in every succeeding experiment, as well as in those already recorded, for the tube almost constantly give way to compression, unless secured by stronger plates on the top side.

Experiment XVI.—August 1st, 1845.

Rectangular tube 18 feet 6 inches long, 18·25 deep, 9·25 wide, and 17 feet 6 inches between the supports.

Thickness of plates, top . . . . $\frac{1·340}{9} = ·1490$ inch.
„ „ bottom . . . $\frac{1·345}{5} = ·2690$ „
„ „ sides . . . . $\frac{0·950}{16} = ·0594$ „

Weight of tube = 317 lbs.
Weight of shackle = 988 lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
988	·15		
2108	·30		
3228	·44	·05	
4348	·60	·07	
5468	·70	·10	
6588	1·00	·22	
6812	......	......	With this weight the top plate began to rise 18 inches from the shackle, after sustaining the weight for about a minute.
∴ Ultimate deflection = 1·03.			Plate XII. fig. 13.

Having in this experiment crippled the upper side of the tube, it was turned upside down, after the injured part was straightened, and the experiment repeated.

In most of the experiments, the tendency to rupture was slow and progressive, a property which seems to be inherent in sheet-iron tubes, particularly when they yield to compression. Under this species of strain, destruction is never instantaneous as in cast-iron; but advances gradually, the material emitting during the process a crackling noise for some time before the experiment is complete, and absolute rupture takes place.

Experiment XVI. *a*.—August 1st, 1845.

The preceding tube reversed, with the thick side uppermost.

Thickness of plates, top . . . $\frac{1·345}{5}$ = ·2690 inch.
„ „ bottom . . $\frac{1·340}{9}$ = ·1490 „
„ „ sides . . . $\frac{0·950}{16}$ = ·0594 „

Weight of tube = 317 lbs.

Weight of shackle = 988 lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
988	·08		
2,108	·30		
3,228	·42	·03	
4,348	·55	·10	* August 2nd.—The weight 11,068 lbs. was left on the tube from half-past 3 o'clock P.M. till the following day at half-past 9 A.M., when the deflection increased from 1·45 to 1·60.
5,468	·70	·15	
6,588	·80	·20	
7,708	·92	·24	
8,828	1·10	·30	* Experiment continued after sustaining the weight 18 hours.
9,948	1·30	·31	
10,508	1·35	·32	
11,068*	1·40	·40	
11,068*	1·60	·50	
11,628	1·65	·53	
12,188	......	......	With this weight the top side puckered.
∴ Ultimate deflection = 1·73.			Plate XII. fig. 14.

The tube failed with 12,188 lbs. at two of the joints on the top side, 3 feet from the shackle. This failure was accompanied by the sides bending inwards on one side, with a similar indentation on the other, and the top plate doubling at the joints in the form of the letter S.

Experiment XVI. *b.*—September 20th, 1845.

The top side still yielding to compression, a stronger plate was riveted on the top side; and in order to cause the bottom to give way to a tensile strain, a thicker plate was riveted over the joint on the bottom side, and the experiment repeated.

Distance between the supports as before, 17 feet 6 inches.

Weight of shackle = 960 lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
960			
2,697	·09		
4,426	·16		
6,173	·25		
7,859	·34		
9,555	·42		
11,262	·60	·07	
12,107	·65	·14	
12,990	·72	·15	
13,867	......	......	Broke after sustaining the weight some minutes by tearing the rivets from the joint on the upper side, 3 feet 8 inches from the shackle.
∴ Ultimate deflection = ·76.			Plate XII. fig. 14.

The great powers exhibited in the last experiments, by the addition of a certain quantity of material to the upper side of the tube, suggested a further extension of the experiments, with some slight modifications of form, in order to render more conclusive the principle which the previous trial had indicated. For this purpose a hollow girder 25 feet long, and 15 inches deep, of the following dimension, was constructed and submitted to experiment.

Experiment XVII. *a.*—August 2nd, 1845.

Rectangular tube or girder, 25 feet 1¼ inch long, 15 inches deep, 2¼ inches wide, and 24 feet between the supports.

Thickness of plates, top . . . . . $\frac{1·300}{5}$ = ·260 inch.
" " bottom . . . $\frac{1·300}{5}$ = ·260 "
" " sides . . . . . $\frac{1·180}{9}$ = ·131 "

Weight of tube = 788 lbs.

Weight of shackle = 800 lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
800	·07	......	The elasticity remains nearly perfect up to 8640 lbs.
1,920	·20		
3,040	·33		
4,160	·50		
5,280	·60		
6,400	·70		
7,520	·83		
8,640	·95	·07	
9,760	1·20	·15	
10,880	1·35	·20	
12,000	1·50	·25	
13,120	......	......	Broke by tearing through the solid plate on the bottom side, 7 inches from the shackle, as the weight was laid on.
∴ Ultimate deflection = 1·613			Plate XIII. fig. 16.

A flaw having been discovered in the plate when the fracture took place from imperfect welding, a stronger plate, 14 inches long and one-fourth of an inch thick, was riveted over the crack, and the experiment repeated.

Experiment XVII.—August 4th, 1845.

Rectangular tube the same as before.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
5,280	·65	·08	
6,460	·77	·15	
7,520	·90	·18	
8,640	1·05	·23	
9,760	1·20	·30	
10,880	1·31	·21	The defects of elasticity were not, from some unknown causes, clearly indicated until the weight 14,240 lbs. was removed, when the defects were found = ·60. Probably this might have arisen from some unequal tension.
12,000	1·46	·21	
13,120	1·60	·21	
14,240	1·75	·60	
14,800	2·11	·62	
15,360	2·17	·62	
15,920	2·28	·68	
16,480	2·36	·74	
17,040	2·38	·80	
17,600	......	......	With this weight the top plate gave way by compression.
∴ Ultimate deflection = 2·66.			Plate XIII. fig. 16.

As this description of beam indicated very considerable powers of resistance, it was deemed advisable still further to test its powers, by allowing the weight 14,240 lbs. to remain suspended during the night. This was done during a period of 17 hours, after which the load was removed. The deflection during that time had increased from 1·75 to 2·00=·25, and the loss in elasticity was ·60 – ·30=·3.

The beam during the two last experiments had suffered considerably from the severity of the strains to which it had been subjected; and it was considered that the anomalous condition of puckering, which had all along been present, might be avoided by reversing the girder with the broad flange uppermost. This was accordingly done, the injured part having first been straightened; and a strong plate 19 inches long having been riveted upon it, the experiment was again proceeded with as follows:—

Experiment XVII. *b*.—August 5th, 1845.

Rectangular tube same as before; the beam reversed with the narrow flange downwards.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
9,760	1·40	·38	The deflection and permanent sets must be added to and subtracted from the respective numbers 1·40 and ·38.
10,880	1·65	·50	
12,000	1·83	·59	
13,120	2·03	·69	
14,240	2·30	·84	
15,360	2·49	·97	
15,920	……	……	Broke when the weight was laid on by extension, the lower plate tearing asunder 6 inches from the centre of the shackle.
∴ Ultimate deflection = 2·58.			Plate XIII. fig. 17.

Owing to the broad flange being placed uppermost, it was expected that the tube would yield to extension, which was the case; but the plate gave way at the rivets of a joint at some distance from the centre. This joint moreover had been a good deal strained by the former experiments, which may account for its fracture by a comparatively less weight.

Experiment XXV.—September 20th, 1845.

Having tested the powers of the larger description of girder in a variety of ways, the smaller one was treated in the same manner, as follows:—

Rectangular girder 12 feet long, 8 inches deep, 1 inch wide, and 11 feet between the supports.

Thickness of plates, top . . . . $\frac{1\cdot41}{5}=\cdot282$ inch.
,, ,, bottom . . . $\frac{1\cdot16}{10}=\cdot116$,,
,, ,, sides . . . . $\frac{1\cdot01}{15}=\cdot067$,,

Weight of tube =125 lbs.

Weight of shackle =930 lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
930	·06		
1,780	·11		
2,630	·16	......	When 9974 lbs. was laid on, the ends from the extreme thinness of the plates were giving way at the supports. Two pieces of hard wood were however inserted between the sides which kept them straight and prevented them from twisting.
3,516	·21		
4,382	·26		
5,214	·32		
6,105	·37	·010	
6,543	·41	·012	
6,996	·44	·035	
7,433	·47	·040	
7,861	·51	·050	
8,273	·54	·058	
8,693	·58	·075	
9,107	·62	·097	
9,545	·67	·118	
9,974	·74	·150	
10,386	·87	·243	
10,827	1·06	·325	
11,254	......	......	The top plate doubled up with this weight 4 inches from the shackle. Finding the top of the beam the weakest, it was turned upside down, and a weight of 6113 lbs. laid on to straighten the injured part.
Beam reversed.			
6,113	·51	·10	
6,549	·60	·13	
6,978	·74	·23	
7,146	......	......	With this weight the top plate was forced upwards.
∴ Ultimate deflection = ·75.			Plate XIV. fig. 24.

This girder, although extremely light on the sides, with a tolerably thick top, nevertheless gave way by compression. Its bearing powers were very considerable with the thick side uppermost, and provided that part had contained a little more material it would have carried upwards of 12,500 lbs.

During the progress of the experiments I had frequent conferences with Mr. Stephenson; and having reported to him from time to time the results that were obtained, and the impression they made upon my mind, he suggested that it might be desirable to have a tube made of an entirely different form, in order, if possible, to throw the top side as well as the bottom of the tube into a state of tension. This suggestion was intended to obviate the anomalous condition of puckering, and to prevent as much as possible that tendency to "buckle," which in every instance is more or less present in rolled sheet-iron plates*. It had a further object in view, namely, to relieve the strain on the centre of the tube, whether arising from the effects of its own weight or the load, by extending the length beyond the supports to a distance of half the span on each side. This additional weight, extending over the piers, was supposed to act as a counterpoise, A A, see Plate XIII. fig. 18, being the fulcrum to that part of the tube in the middle at B, and it would also assist in the support of the load during the passage of the trains through the tube. For these objects a tube was made of the form shown in Plate XIII. fig. 18.

Experiment XVIII. *a*.—August 3rd, 1845.

Rectangular tube 37 feet 8 inches long, 13·25 inches deep in the middle, 7½ inches wide, with the upper part raised to 17·25 inches, as at A A, and 18 feet between the supports. The width of the top and bottom plates as per section below.

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* It is almost next to impossible to roll plates with all the parts in the same degree of tension. Almost every plate has more or less "buckle," and it requires no inconsiderable degree of skill to elongate those parts of a plate where the tension is greatest, and also to find out the parts which cause the "buckle." A considerable portion of variable tension in the composition of plates is probably caused by unequal contraction in the process of cooling, and also by a difference of temperature in the blooms from which the plates are rolled.

Thickness of plates, top . . . . $\frac{1·140}{8}=$ ·1425 inch.
„ „ bottom . . $\frac{1·140}{8}=$ ·1425 „
„ „ sides . . . $\frac{1·240}{11}=$ ·1127 „

Weight of tube =640 lbs.

Weight of shackle =800 lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
800	·09		
1,920	·20	·02	
3,040	·32	·05	
4,160	·45	·09	
5,280	·59	·16	
6,400	·71	·19	
7,520	·84	·22	
8,640	·99	·27	
9,760	1·18	·32	
10,880	......	......	With this weight the top plate doubled up 1 foot 6 inches from the shackle.
∴ Ultimate deflection = 1·31.			Plate XIII. fig. 18.

After the top side had yielded to compression the weights were removed, and the supports under the bottom having been lowered, the tube was supported on two cross bars passing through the tube at A A, and the weights were again suspended upon it.

These weights when laid on made no difference in changing the direction of the forces, as the top plate was again forced upwards by compression, and that to a greater height than before, accompanied with increased distortion of the sides, which shortly became collapsed diagonally on each side of the shackle.

In this experiment it is probable that some degree of tension was induced along the upper line of the top plate, as the extreme end of the tube was raised with some force as the weights were increased. This property of the weight raising the end over the fulcrums A A was strikingly apparent when the whole weight, 10,800 lbs., was laid on; but it did not appear to alter the conditions of the middle part, which was forced upwards by compression, and followed the same law as if it had been formed of a single beam.

These appearances indicated a tendency of the two ends, when extended to half the distance between the supports, to act as a counterpoise, and not only to change the direction of the strain on the top side, but relieve the bottom, which in other respects must have borne the whole weight. From this it is inferred, that the tube in its full size would be greatly relieved by increasing its length on each side of the land towers, as in the case of the Britannia Bridge, to the extent of half the span.

As a further illustration of these views, the injured part of the tube was repaired by riveting an additional plate of the same thickness along the top side over that previously damaged. With this addition the experiment was again repeated.

Experiment XVIII. repeated.—August 10th, 1845.

Rectangular tube same as before.
Thickness of plates, top . . . . . =·2850 inch.
" " bottom . . . . =·1425 "
" " sides . . . . . =·1127 "
Distance between the supports 18 feet.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
7,520	·68		
8,640	·84	·04	
9,760	·99	·10	
10,880	1·15	·16	
12,000	1·31	·25	With this weight, 1200, the sides were slightly puckered, indicating a tendency to force the top side upwards.
12,560	1·40	·32	
13,120	1·64	·42	
13,680	......	......	Puckered as before by compression, the top plate doubling up 13 inches from the shackle.
∴ Ultimate deflection = 1·71.			Plate XIII. fig. 18.

On consulting the two last experiments, it is obvious that no great increase of strength was obtained by doubling the thickness of the top plate. This may, however, be accounted for by the circumstance of the top plates being under instead of above the line of compression. In every description of girder composed of malleable iron, and probably of any other material, the upper side should be elevated to a greater height than the line of ultimate deflection. It should always be above, but never below the horizontal line of compression. Another cause of the failure of this tube, with a comparatively less weight than the increased thickness of the top would indicate, might be traced to the severe injuries which that part sustained in the previous tests. Hence followed the puckering of the top side, at a much earlier stage of the experiment than it should have done, had the plates been sound and the line of the forces changed.

The experiments on this form of tube are perhaps the more interesting from the fact, that they exhibit certain defects of form, which it may be desirable to avoid in girders of this description. If the parts suspended beyond the piers are intended to act as a counterpoise to the load, it will then become necessary to have the girder of uniform strength and texture, with a slight curvature of the top side about $\frac{1}{10}$th the depth. With these precautions in the construction, the strength would be greatly improved, and being subjected to severe strain, will follow the same law, as regards extension and compression, as those of a girder of the simple form.

During the progress of Experiment XXII., when the elliptical tube, after being strengthened by an iron cellular fin riveted along the top, was found defective in resisting the crushing force, it then occurred that a different construction might be introduced so as to give strength and rigidity to that part. For this purpose, I sketched out, and gave orders for, the construction of a tube with a corrugated top, forming two longitudinal cavities along the whole of its top side, as exhibited in the sketch, Plate XIV. fig. 27.

This tube was executed with considerable care, and having been submitted to the usual experimental test, the results were as follow :—

Experiment XXIX.—October 14th, 1845.

Rectangular tube with a corrugated top, 19 feet 8 inches long, 15·4 inches deep, 7·75 inches wide, and 19 feet between the supports.

Thickness of plates, top . . . $\frac{\cdot 23}{2}$ = ·115 inch each.
„ „ bottom . . = ·180 „ „
„ „ sides . . . = ·070 „ „

Weight of tube = 500 lbs.

Weight of shackle = 988 lbs.

The tubes *a a* were 1·65 inch diameter.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
988	·035		
2,736	·110		
4,468	·190		
6,215	·270		
7,924	·340	·020	
9,636	·424	·050	
11,334	·523	·062	
13,041	·640	·095	
14,751	·735	·125	With the weight 18,205 lbs. the deflection increased ·02 inch in three minutes.
16,490	·870	·186	
18,205	1·070	·276	
19,065	1·155		
19,918	1·270	·400	
20,764	1·425		
21,629	1·520	·590	
22,469	……	……	Broke by the side plate tearing from the top at 2 feet from the shackle.
∴ Ultimate deflection = 1·59.			Plate XIV. fig. 27.

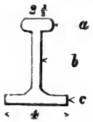
A short time previous to the tearing of the sides from the top at the rivets, that part had begun to assume a slightly undulating appearance on one side, arising from the weakness of the side plate, which gave way near to the shackle. This was not, however, the only part that suffered under the strain, as the opposite side was tearing from the bottom plate at the same time, evidently showing a rapid approach to rupture on both the lower and upper sides of the tube. These parts exhibited important features in the due and perfect adjustment of the top and bottom, which in this case were calculated to resist, as nearly as possible, the forces acting upon them.

Another property of considerable importance, in this description of girder, is its progressive tendency downwards to destruction. It is widely different from cast-iron and other crystalline substances in this respect, since, from its fibrous nature and greater ductility, it gives timely warning before rupture takes place. This property was noticed in several of the former experiments; and in this experiment, it became more apparent after the whole weight 22,469 lbs. was laid on. With this weight, more than three minutes elapsed before the experiment was completed and the tube rendered unfit for use.

Experiment XXX.—October 10th, 1845.

On a malleable iron beam, of the annexed sectional form, 11 feet 7 inches long, and 11 feet between the supports.

Fig. 66.



Dimensions at $a = 1\cdot000$ in. × $2\frac{1}{2}$ in.
,, ,, $b = \cdot325$ in. × 7 in.
,, ,, $c = \cdot380$ in. × 4 in.

Weight of beam = 227 lbs.

Weight of shackle = 885 lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
885	·04		
2,581	·12		
4,317	·20		
6,050	·26		
7,743	·35		
9,493	·46		
11,253	·60	·09	
12,955	......	......	With this weight the beam became distorted, and continuing the weight for some time, the deflection kept increasing until it bent laterally so as to be no longer able to sustain the load.
∴ Ultimate deflection = ·69.			Plate XIV. fig. 25.

Experiment XXXI.—October 10th, 1845.

On a malleable iron beam, of the annexed sectional form (see fig. 66), 10 feet 8 inches long, and 10 feet between the supports.

Dimensions at $a = 1{\cdot}000$ in. × $2\frac{3}{7}$ in.
„ „ $b = {\cdot}350$ in. × 8 in.
„ „ $c = {\cdot}440$ in. × 4·30 in.
Weight of beam = 247 lbs.
Weight of shackle = 885 lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
885			
2,631	·04		
4,358	·12		
6,098	·15		
7,827	·19		
9,585	·21		
11,278	·26		
12,980	·30	·03	
14,693	·35	·03	
16,373	·45	·09	
18,115	·68	·26	
18,962	......	......	With this weight the beam was distorted and the experiment discontinued.
∴ Ultimate deflection = ·71.			Plate XIV. fig. 26.

In both these experiments the beams yielded to lateral deflection, showing certain defects of form arising from want of lateral strength and breadth in the top and bottom flanges.

Experiment XXXII.—October 10th, 1845.

Malleable iron beam of the same form as the last, 10 feet 7 inches long, and 10 feet between the supports.

Thickness $a = 1{\cdot}000$ in. × 2·75 in.
„ $b = {\cdot}380$ in.
„ $c = {\cdot}420$ in. × 4·30 in.
Weight of beam = 276 lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
885	·020		
2,606	·050		
4,364	·090		
6,105	·110		
7,835	·140		
9,559	·165	·03	With 21,553 lbs. the deflection increased in four minutes ·025, in the next four minutes ·10, and in four minutes more it had sunk to ·34.
11,257	·195	·03	
12,999	·220	·04	
14,728	·250		
16,407	·250		
18,108	·290		
19,839	·370		
21,553	·475		
22,387	·590		
23,046	......	......	Bent laterally upwards of 2·65 inches, when the experiment was discontinued.
∴ Ultimate deflection = ·6.			

The above was the last trial made upon solid beams. They are obviously much inferior in strength to the hollow rectangular girders.

The preceding experiment was completed on the 14th of October 1845, and from that time till the beginning of July following, little or nothing was done. An abridged report, giving a summary of results from the experiments, was read to the Directors of the Chester and Holyhead Railway Company. That report is now before the public; and from the satisfactory nature of the results therein recorded, and those in particular obtained from the tube with the corrugated top, the Directors were induced, through the recommendation of Mr. Stephenson, to adopt this description of bridge in preference to others of a less practical character. It was nevertheless considered necessary to make some further experiments on a larger scale, in order to determine the form and proportions of the tubes. For these objects an entirely new model tube, exactly *one-sixth* the dimensions of the Britannia Bridge, was constructed, and having arranged the apparatus, as shown in the annexed figure, the experiment was proceeded with as before.

Apparatus for Experimenting on the Model Tube.

Fig. 67.

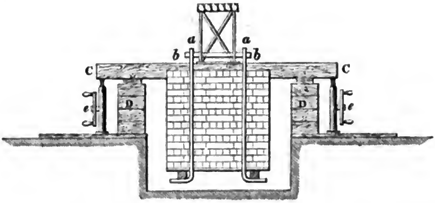


Fig. 68.

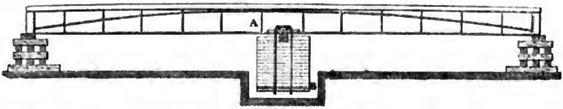


Fig. 68 represents at A a side view of the model tube, 75 feet between the supports; B is the platform, supporting the weights, suspended from two cross-bars passing through the sides and resting upon the outside angle-iron and the bottom of the tube, as shown in the section, fig. 67, at *a*, *a*. C C is a balk of timber passing under the cross links *b*, *b*, which on being raised by two powerful screw-jacks *e*, *e*, lifted the load from off the tube, in order to ascertain the effects of the successive loads upon the elastic powers of the tube. The temporary supports D D were only used for the purpose of receiving part of the falling load, in case of accident or the rupture of the tube.

Model tube.—Experiment XXXIII.—July 10th, 1846.

Rectangular tube 78 feet long, 4 feet 6 inches deep in the middle, 2 feet 8 inches wide, and 75 feet between the supports, being as nearly as possible one-sixth the dimensions of the proposed tubes across the Menai Straits.

$$\text{Thickness of plates, top}\left\{\begin{array}{ll}1\text{ upper plate} & \frac{1\cdot05}{7}\\ 1\text{ division plate} & \frac{1\cdot12}{8}\\ 1\text{ lower plate} & \frac{1\cdot17}{7}\end{array}\right\}=\cdot147\text{ inch.}$$

Thickness of plates, bottom . . . . . $\frac{1\cdot26}{7}=\cdot180$ inch.

„ „ sides . . . . . $\frac{\cdot99}{10}=\cdot099$ „

Weight of tube $=10888\cdot94$ lbs.

Weight of shackle and platform for supporting the weight $=2037$ lbs.

Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
2,037	·175		
4,571	·175		
7,082	·275		
9,596	·375		
12,136	·485		
14,672	·575		
17,214	·635	·035	
19,725	·800	·035	
22,093	·925	·075	
24,264	1·045	·086	
26,415	1·140	·100	
28,519	1·235	·105	
30,632	1·335	·115	
32,769	1·450	·150	
34,937	1·560	·200	
37,079	1·660	·245	
39,260	1·780	·290*	* At this point the screw-jacks were no longer able to raise the weight, consequently the defects of elasticity could not be taken until the load 68,378 lbs. was removed.
41,476	1·900		
43,602	2·000		
45,811	2·100		The weight 56,577 lbs. was suspended for one hour upon the tube, which caused an increase of deflection from 2·700 to 2·850.
47,939	2·200		
50,071	2·300		
52,253	2·400		With the weight 68,378 lbs. some change took place, arising probably from part of the strain being removed during the night.
54,434	2·525		
56,579	2·700		
58,729	2·930		On the following morning the weights were entirely removed, when the permanent set was found to be ·806, as marked in the table.
60,911	3·000		
63,059	3·200		
65,564	3·400		
67,842	3·575		
68,378	3·425	·806	
70,618	3·625		
72,858	3·726		
75,098	4·050		
77,338	4·375		
79,578	......	......	Broke after sustaining the load a minute and a half, the bottom plate tearing asunder 2 feet from the centre of the shackle.
∴ Ultimate deflection = 4·5.			Plates XV., XVI. and XVII. figs. 28, 29, 30, 31, 32, 33, 34, & 35.

In the construction of the model tube, from which the preceding and succeeding experiments were derived, the cellular top was at first nearly three times the area of the bottom, with a view of enabling us to increase the area of the section of the latter part, until an equilibrium of resistance was attained between the forces of compression and extension. At every succeeding fracture, the strength of the bottom was increased, until its resistance to extension became greater than the resistance of the cellular top to compression. The repairs upon the bottom part of the tube would evidently cost much less labour and expense, than any repairs that might have been required upon the cellular top, which accounts for the excess of strength in that part.

In the last experiment, the comparative areas of the top, bottom and sides were as follow:—

Area of the top . . . . . =24·024 inches.
Area of the bottom . . . = 8·800 ,,
Area of the two sides . . = 9·000 ,,

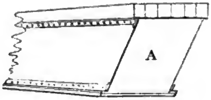
Immediately after this experiment, the tube was repaired and strengthened by two additional plates, each $6\frac{1}{2} \times \frac{5}{16}$th of an inch thick; and having repaired the injured parts of the top cells, and renewed the fractured plates below with an increased area of the bottom =12·8 inches, the experiment was again proceeded with as before.

Experiment XXXIV.—July 31st, 1846.

Rectangular tube (model tube) one-sixth of the dimensions of the proposed bridge.

Total length of tube 78 feet. Breadth 2 feet 11 inches. Depth 4 feet 6 inches. Length between supports =75 feet.

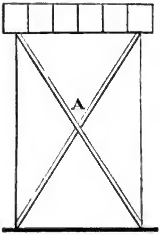
Thickness of plates, bottom plates . . =0·156 inch.
,, ,, side plates . . . . =0·099 ,,
,, ,, upper top-plates .
,, ,, lower top-plates . =0·147 ,,
,, ,, division plates . .
Area of the top . . . . =24·024 ,,
Area of the bottom . . . . =12·800 ,,
Weight of tube =1,088,894 lbs.

No. of experiments.	Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
1	2,050	0·30		Fig. 69. 
2	13,707	0·40		
3	17,470	0·53		
4	21,258	0·74		
5	24,595	0·90	0·14	
6	27,211	1·01	0·14	
7	29,837	1·12	0·16	
8	32,431	1·22		
9	34,985	1·32		
10	37,571	1·43		
11	40,157	1·54		
12	42,757	1·63		
13	45,337	1·73	......	This weight, 45,337 lbs., was left suspended upon the tube for sixteen hours, when the deflection was again taken, and found still the same, viz. 1·73. From this it appears, that all the parts of the tube under strain had become permanent as regards their power of resistance to the load.
14	48,383	1·82		
15	50,945	1·89		
16	53,501	1·98		
17	56,083	2·08		
18	58,746	2·18		
19	61,316	2·28		
20	63,917	2·37		
21	66,518	2·46		
22	69,141	2·56		
23	71,821	2·65		
24	74,366	2·79		
25	76,951	2·86		
26	79,979	2·99		
27	82,544	3·09	......	With this weight (82,554 lbs.) the deflection increased in forty-five minutes from 3·09 to 3·22.
28	85,026	3·31		
29	87,510	3·39		
30	89,844	3·50		
31	92,113	3·62		
32	93,769	3·76		
33	95,432	3·93		
34	97,102	......	......	The tube gave way at one end by twisting out of the perpendicular, as shown in sketch at A, after sustaining the load 97,102 lbs. a few seconds.
∴ Ultimate deflection = 0·4.				Plates XVIII. and XIX. figs. 36, 37, 38, 39 and 40.

From the above experiment, it is obvious, that failure was not caused from want of strength in either the top or the bottom, but from want of stiffness in the sides. This was not so perceptible along the sides of the tube, as it was at the ends where it rested on the supports; and provided a strong iron frame had been introduced, such as was afterwards applied, it is probable, that the experiment might have been continued till rupture took place, as in the preceding experiment.

The tube having been greatly distorted by one end twisting, as shown in the preceding experiment, it was again strengthened and repaired by the introduction of vertical bars of 1½ angle-iron, riveted to the sides (internally) at every 2 feet. A St. Andrew's cross, as shown at A, fig. 70, was also inserted at each end. With these repairs, no additional strength having been introduced, the experiments were resumed August 22nd, as follows :—

Fig. 70.



Experiment XXXV.—August 22nd, 1846.

The same tube as in preceding experiment. The sectional areas in the middle, as follows :—

Area of top . . . . . = 24·024 square inches.
,, bottom . . . = 12·800 ,, ,,
,, the two sides . = 4·5 × 2 = 9·000 ,,

No. of experiments.	Weight in lbs.	Deflection in inches.	Deflection, load removed.	Remarks.
1	22,861	0·875		
2	36,179			
3	47,467	1·850		
4	58,875	2·850		
5	70,722			
6	77,527	3·250	......	This weight was left on nineteen hours, when the deflection increased from 3·250 to 3·500.
7	82,172	3·83		
8	86,813	3·97		At this point the experiment was discontinued, and 20 tons was left suspended upon the tube from this date, August 22nd to September 10th, when the deflections were again taken, and were found to have increased from 3·5 to 3·75 inches.
9	91,411	4·13		
10	96,092	4·32		
11	98,412	4·40		
12	100,748	4·49		
13	103,070	4·62		The experiments were then resumed, beginning with 82,172 lbs.
14	105,391	4·69		
15	107,714	4·82		
16	110,033	4·94		
17	112,216	5·10		
18	114,525	5·27		
19	116,759	5·38		
20	119,169	5·50		
21	121,443	......	1·96	Here the platform sustaining the weights gave way and stopped the experiments. September 11th. The platforms having been restored, the same weight was again laid on, and the experiment continued.
22	123,724	5·68		
23	126,128	......	......	Broke with this weight, 56·3 tons, the plates tearing asunder at a distance of inches from the shackle.
∴ Ultimate deflection = 5·79.				

It will be necessary here to remark, that after the failure of the platform on which the weights were suspended, and previous to the completion of the experiment, it was desirable to ascertain the defects of elasticity. The whole of the weights, 121,443, were accordingly removed, when it was found that the tube restored itself within 1·96 inch of its original form, equal to a permanent set of nearly 2 inches.

If we consider the position of the tube and the close approach upon fracture, when the suspended weights and platform gave way, it will appear evident, that riveted plates, when subjected to a transverse strain, have no inconsiderable claim to attention, either as regards their elasticity or their powers of resistance to severe strain. In the above experiment, it will be seen, that although verging on rupture, the tube still retained an elastic force of $5·50-1·96=3·54$ inches.

On a careful inspection of the top of the tube previous to laying on the last weights, an appearance of failure was perceptible. This was confined to the rivets of the top plates, which were cut, as if sheared by a sharp instrument. These appearances may however be attributed to a mistake made by the foreman in making *lap*, instead of *butt* joints of the plates which covered the cells.

In the last experiment, the tube was torn asunder through the bottom, with a weight of 56¼ tons, and the cellular top having continued to exhibit unimpaired powers of resistance, it was deemed necessary to repair the injured parts a third time. It was further deemed expedient to rivet two stronger slips along the bottom, 20 feet on each side of the centre, in order to enlarge the sectional area in the middle, and to force the top to yield to compression. These strips were 9 inches wide and half an inch thick, which increased the area of the bottom from 12·8 to 17·8 inches. The injured part of the sides and top were also restored, but without making any addition to the strength of those parts. The tube having been thus repaired, the experiment proceeded as follows:—

Experiment XXXVI.—October 16th, 1846.

Area of the top . . . =24·024 inches.
Area of the bottom . . =17·800 „

No. of experiments.	Weight in lbs.	Deflection in inches.	Deflection with load removed.	Remarks.
1	2,037			
2	61,649	1·50		
3	65,892	1·62		
4	70,116	1·73		
5	74,363	1·88	......	74,363 lbs. were left on for forty minutes without any perceptible change.
6	78,846	2·01		
7	83,911	2·17		
8	88,682	2·33		
9	93,338	2·48		
10	99,131	2·72		
11	103,803	2·87	......	About 45 tons were left suspended on the tube for twenty-one hours, but without any increase in the deflection.
12	108,370	3·03		
13	113,090	3·22		
14	117,661	3·40		
15	122,397	3·63	......	Here the deflection increased from 3·63 to 3·70 in five minutes, when it became fixed.
16	127,045	3·87		
17	129,178	4·01		
18	131,521	4·12	......	With 127,045 lbs. the cellular top gave slight indications of puckering at the top edge on both sides.
19	133,876	4·27		
20	136,150	4·37		
21	139,304	4·62		
22	141,579	4·73		
23	143,727	4·81		
24	145,923	4·88		
25	148,129	......	......	Broke with this weight, the bottom tearing asunder through the solid plates. The sides were also damaged, as shown in Plate XVII. figs. 33, 34 and 35. On a careful examination of the fracture it was found that the 9-inch strips were very defective, having more the appearance of cast than wrought-iron.
∴ Ultimate deflection =4·94.				

From a slight settling of the ground, where the heavier weights were laid on, some doubts were entertained as to the accuracy of the deflections taken in this experiment; and in order the more effectually to determine the error, if any, two different methods were resorted to; one by weights attached to a fine line at each end of the tube, and the other by a fine wire, which moving over a pulley at one end kept them in the same uniform state of tension. These two methods were subsequently adopted, and when any difference existed, which was seldom the case, I generally took the mean of the two observations.

The number of times that the tube had been repaired and restored to its original form, furnished grounds for hope, that the last addition to the bottom would have crushed the cellular top, and enabled us finally to have closed the experiments. The results were however different from our expectation, the bottom having yielded a third time to tension: we were again compelled to repair the damage, by cutting out the fractured parts, and renewing them in the bottom with stronger and better plates.

In every case, the rupture of the bottom plates considerably damaged the top cells, as well as the sides; in consequence of this, the repairs did not merely consist in making new plates for the bottom, but also in the straightening of the sides and upper cells; and sometimes even in the renewal of the injured parts. In the present instance an entirely new bottom, extending 20 feet on each side of the shackle, was introduced. This bottom was composed of double plates, one-fourth of an inch thick, with two strips along the middle as before; by these repairs the area was increased in the middle from 17·8 inches to 22·45 inches. The other damages having been repaired the experiments were again continued.

Experiment XXXVII.—December 7th, 1846.

Area of the top . . . . =24·024 inches.
Area of the bottom in the middle . =22·450 sq. inches.
Weight of tube 5 tons. 16 cwt. 1 qr.

No. of experiments.	Weight in lbs.	Deflection in inches.	Deflection with load removed.	Remarks.
1	2,037	0·00		
2	59,123	1·02		
3	76,204	1·75		
4	87,189	1·98		
5	95,172	2·20		
6	104,568	2·45		
7	114,123	2·70		
8	124,203	3·08		
9	129,007	3·20	......	129,007 lbs. were left suspended on the tube for eighteen hours, when the deflection was again taken, and found to have increased from 3·20 to 3·35 inches.

At this stage, it was deemed advisable to discontinue the experiment until the tube was laid on its side for the purpose of ascertaining its lateral strength, and also for determining the resistance which tubes of this construction would offer to the action of the wind. For these objects the weights were removed, and in doing so, great attention was paid to the deflections as each layer of weights was taken off the scales. This was done for the purpose of ascertaining the elastic powers of the tube, which, taken inversely in the order of the weights as they were laid on in the last experiment, were as follows :—

No of experiments.	Weight in lbs.	Deflection in inches.	Deflection with load removed.	Remarks.
1	129,007	3·35		
2	113,847	3·15		
3	104,263	3·00		
4	94,410	2·83		
5	85,071	2·61		
6	74,644	2·43		
7	61,078	2·08		
8	49,700	1·83		
9	38,577	1·58		
10	25,619	1·22		
11	13,204	0·92		
12	9,937	0·45		
	0,000	0·43	……	The permanent set, or loss of elasticity, is therefore only 0·43 inch.

At the very commencement of the experiments on the model tube, it was intended to set at rest, by direct experiment, the question so often mooted as to the effects of the wind upon the tube; and the present appearing to be a good opportunity for solving the problem, it was considered advisable, after subjecting the tube to a weight of 58 tons for eighteen hours, to lay it upon its side, and test it in the usual way. It was accordingly laid flat upon the blocks, and with the same shackle and platform suspended from the upper side; the experiment proceeded after the manner shown in the following table :—

Experiment XXXVIII.—December 9th, 1846.

The tube laid on its side.

No. of experiments.	Weight in lbs.	Deflection in inches.	Deflection with load removed.	Remarks.
1	2,172	0·08		
2	6,592	0·28		
3	9,103	0·62		
4	11,648	0·85		
5	13,880	1·02		
6	16,048	1·15		
7	18,204	1·43		
8	20,349	1·65		
9	22,467	1·81		
10	24,646	2·29		
11	26,781	2·36	......	The weight 26,781 lbs. was left suspended upon the tube for some hours, when the deflection was again taken, and found to have increased by 0·14, or from 2·36 to 2·5 inches.

Previous to the application of the weights on the sides of the tube, when laid flat upon the supports, the deflection was found to be ·85 inch with its own weight. With 12 tons this deflection was increased to ·85+2·36=3·21 inches, the amount of deflection due to $\frac{5}{8}$ths of this weight of the tube and the load, or about 15 tons. After these tests the tube was restored to its proper position, and having applied the weights as before, the defects of (lateral) elasticity were found not to exceed *one-tenth of an inch.*

Adding $\frac{5}{8}$ths of the weight of the tube to the numbers in the column of weights in the preceding table, we have

No. of experiments.	Deflecting weight in lbs.	Deflection in inches.
5	22,017	1·87
7	26,341	2·28
9	30,583	2·66
11	34,918	3·21

Putting δ = the deflection in inches, and W = the deflecting weight in lbs., then we find that the formula

$$\delta = \frac{W}{9600} - \cdot 43$$

nearly represents the relation of the deflections and weights in the last table.

Now taking the constant for the tube in Experiment XIV. (p. 271) to apply to the present case, we have by formula (5.), p. 275,

$$W = \frac{AdC}{l} = \frac{55{\cdot}47 \times 4{\cdot}5 \times 11{\cdot}7}{75} = 38{\cdot}9 \text{ tons} = 87176 \text{ lbs.}$$

Hence, by the foregoing formula, we have for the ultimate deflection of the model tube, placed upon its side,

$$\delta = \frac{87176}{9600} - {\cdot}43 = 7{\cdot}55 \text{ inches.}$$

Having attained satisfactory results as to the lateral strength of tubes of this construction, the tube was restored to its vertical position and again loaded, as in the following table.

Experiment XXXIX.—December 14th, 1846.

The tube raised to its former vertical position.

No. of experiments.	Weight in lbs.	Deflection in inches.	Deflection with load removed.	Remarks.
1	2,240	0·00		
2	11,568	0·70		
3	20,169	1·31		
4	32,133	1·91		
5	54,185	2·38		
6	79,548	2·77		
7	105,883	2·96		
8	135,255	3·17	......	This weight was left suspended on the tube for nine days and nights, viz. from 12 o'clock on the 14th till the same hour on the 23rd, when the deflection was found to have increased from 3·17 to 3·22 inches.

The object for which the first portion of the experiment was made, was to ascertain the effects produced upon the tube by long-continued strain. This experiment was analogous to those made upon cast-iron for the British Association for the Advancement of Science*.

The effect of time upon material when subjected to severe strain, and that more particularly when the load approaches the point of fracture, has long been a question of doubtful solution. In subjecting any body to a pressure, which carries it beyond what is denominated the elastic limit, it is then considered unsafe to load it to any greater extent. At that point the body has taken a permanent set, its atoms or fibrous structure are changed, and on the whole it is considered prudent that the permanent load should not exceed this limit.

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* Transactions of the British Association, Mr. Fairbairn's Report, vol. vi.

Now, according to experimental facts recently ascertained, this view of the subject appears to be a mere assumption, as the experiments on the effects of time alluded to, clearly establish the fact, that bodies may be loaded (instead of to one-third of the breaking weight, which is the assumed elastic limit) to within $\frac{1}{12}$th of the load requisite to produce rupture and without endangering their security. Even at this point, the body will continue to support the load (in the absence of any disturbing cause) with undiminished energy as respects its powers of resistance.

These important facts tend to increase our confidence in the bearing powers of bodies, and may further be useful in the advancement of physical science as applied to the arts. The position of an elastic limit is exceedingly indefinite, as the experiments conducted by Mr. Hodgkinson and myself have sufficiently shown*. In these experiments it is established, that the elasticity of bodies is not only affected by a comparatively small weight, but what is more than probable, every minute weight will produce a change in the molecular structure of the body, as well as in its powers of elasticity.

Experiment XL.—December 23rd, 1846.

Tube the same as in Experiment XXXIX.

No. of experiments.	Weight in lbs.	Deflection in inches.	Deflection with load removed.	Remarks.
1	135,255	3·22	......	Weight left suspended on the tube since December 14th.
2	139,567	3·38		
3	144,352	3·48		
4	149,684	3·70		
5	151,772	3·81		
6	154,452	......	......	Broke by tearing asunder through the bottom at the end of new plates, 21·6 inches from the shackle. Area of the bottom at the point of fracture 8·8 inches.
∴ Ultimate deflection = 3·86.				

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* Transactions of the British Association, vol. vi.

In the preceding experiment there is sufficient evidence to show, that the point where the new plates terminated was the weakest part of the tube, and that the cause of fracture did not arise from *the weakness of the sides, nor yet from the cellular top*; but from a want of due proportion in the material which composed the bottom of the tube, and hence followed rupture at the point of greatest weakness.

These views are sufficiently apparent from the fact, that the sides and cellular top were never increased in strength from the commencement, and they sustained no injury, excepting only what was occasioned by the falling of the tube on the blocks, when the bottom was ruptured. It will further be observed, that the plates of the sides and cellular top were of equal thickness throughout, and had to sustain a greater strain in the middle than in any other part, clearly showing that the sides and top were the strongest parts of the tube. Another circumstance corroborative of this fact is, that the tube in the last experiment gave way with a deflection of 3·81 inches; whereas in some of the previous experiments, a deflection of upwards of 5½ inches was attained before the bottom was torn asunder. Now if the sides or top had been the weakest parts of the tube, they must of necessity have been the first to give way, a circumstance which never occurred, and hence the inference,—that the relative proportions of the top, bottom and sides, are rapidly approaching the section of greatest strength*. It will be borne in mind, that the experiments recorded in the preceding and following tables are upon a large scale; and assuming that the tubes for the Britannia and Conway Bridges follow the same law as that of the model tube, as regards their powers of resistance to a transverse strain, we should then infer that we have sufficient strength in the large tubes either as regards the weight of traffic or the strength of the wind.

The new plates in this experiment did not extend beyond

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* These remarks were written immediately succeeding the last experiment.

21 feet on each side of the shackle, which left the remaining portion of the bottom between those points and the supports on each side in their original form, viz. 8·8 inch area. It is to be regretted that the new plates, $\frac{1}{4}$th of an inch thick, did not extend a few feet nearer the supports, as it would have increased the bearing powers of the tube and prevented the rupture which took place, as recorded in the table with a weight of 69 tons.

The repeated fracture of the bottom of the tube caused it again to be repaired with additional plates, extending a few feet nearer the supports. This being accomplished, and the sides and top having again been restored, the experiment proceeded as before.

Experiment XLI.—April 15th, 1847.

Area of the top . . . . . =24·024 inches.
Area of the bottom . . . . =22·450 „

No. of experiments.	Weight in lbs.	Deflection in inches.	Deflection with load removed.	Remarks.
1	20,006	0·55		
2	35,776	0·78		
3	48,878	1·12		
4	62,274	1·48		
5	77,534	1·78		
6	92,299	2·12		
7	103,350	2·38		
8	114,660	2·70		
9	132,209	3·05		
10	138,060	3·23		
11	143,742	3·40		
12	148,443	3·58		
13	153,027	3·70		
14	157,728	3·78		
15	161,886	3·88		
16	164,741	3·98		
17	167,614	4·10		
18	171,144	4·23		
19	173,912	4·33		
20	177,088	4·47		
21	180,017	4·55		
22	183,779	4·62		
23	186,477	4·72		
24	189,170	4·81		
25	192,892	......	......	Broke with this weight (86⅛ tons), the cellular top puckering at a distance of 2 feet from the shackle, the bottom and sides remaining uninjured.
∴ Ultimate deflection = 4·89.				Plate XX. figs. 41 and 42.

The crushing of the cellular top attained the object we had been in search of, from the commencement of these experiments. It was therefore highly gratifying to find ourselves in a position that no longer admitted of doubt. This experiment determined the relative proportions of the top and bottom areas of the tube so as to balance the forces of compression and extension, developed by a transverse strain, and furnished also other data necessary for the construction of a tube, having a maximum strength with a given quantity of material. Having obtained these important data, it became expedient to close the experiments, and from these data to deduce such formulæ as will enable us to compute the strength and proportions of any other tube subjected to a transverse strain.

The next and closing experiment is of a gigantic character, namely, upon the large tube itself. It was resolved, in order to make security doubly sure, that an experiment should be made upon the first of the Conway tubes; that temporary piers should be built under it at each end (corresponding with the span of 400 feet); and that the deflections should be taken with variable and increasing loads, in the usual way. Immediately on the completion of the tube this was done; the scaffolding, platforms, &c. having been removed, the tube was suspended, and the experiment proceeded as follows:—

Experiment XLII.

Rectangular tube 412 feet long, 25 feet 6 inches deep in the middle, 15 feet wide, and 400 feet between the supports.

Area of the top . . . . . . =670 inches.
Area of the bottom . . . . . =517 „

Computed weight of tube, including rails and cast-iron frames at the ends, 1300 tons.

No. of experiments.	Distance between supports.	Depth.	Width.	Deflection.	Weight.	Remarks.
	ft. in.	ft. in.	ft. in.	in.	tons.	
1	400 0	25 6	15 0	7·91	0	The weight of the tube, 1300 tons, gave a deflection of nearly 8 inches, and 95 tons left in the inside of the tube for four hours, increased the deflection from 9·02 to 9·25 = ·23 inch. This weight was continued for seventeen hours, with an increase of deflection ·10 inch. After this 301 tons, exclusive of the weight of the tubes, were laid on, when the experiment was discontinued.
2	400 0	25 6	15 0	9·02	95	
3	400 0	25 6	15 0	9·50	154	
4	400 0	25 6	15 0	10·50	201	
5	400 0	25 6	15 0	10·95	301	

An experiment upon the full-sized scale was deemed the most satisfactory method of arriving at correct conclusions.

The first weight, 95 tons, was spread over a surface of 70 feet in the length of the tube in the middle.

The second weight was laid over a surface of 105 feet in the middle.

The third over a surface of 150 feet in the middle.

The last over a surface of 190 feet in the middle.

On the 27th of February 1848, 10 A.M., it blew a severe gale from the N.W., which impinged upon the side of the tube at an angle of about 50°. This produced an oscillating motion, which being carefully measured, gave a lateral deflection of ·23 inch. Towards noon the wind blew in gusts, but the lateral effect upon the tube, although quite perceptible, did not exceed one quarter of an inch.

The result of this experiment fully confirmed the preconceived opinion relative to the lateral stiffness of the structure, which exhibited then, as it does now, considerable powers of resistance to lateral strain; and taking into consideration the great weight of the tube, its cellular structure on the top and bottom platforms, and the combination of its parts, there existed no doubt as to the lateral security of the bridge.

For some time after the first Conway tube was erected, considerable attention was paid to the effects produced by the heat of the sun's rays upon so large a surface of iron. About sunrise, on a clear frosty morning, the gauges with graduated scales were set, and after marking the correct position of the tube, the following results were obtained:—

At noon, the sun shining bright upon the top and one side, the tube was bent in that direction, ·96 inch, or nearly 1 inch. Comparing the deflections as they stood in the morning, before sun-rise, and at noon when the temperature had greatly increased, a decrease of $\frac{7}{10}$th of an inch, or a rising of the middle of the tube to that extent, had taken place*. The result of this change of temperature was an increased convexity in the curvature of the top and bottom sides, to the extent of raising the whole tube ·71 inch in the middle. The effect of the sun's influence was therefore considerable, and the infusion of heat into the top had, by the elongation of that part, raised the whole weight of the tube to a height of three-fourths of an inch, besides an elongation of nearly three-fourths of an inch.

Summary of Results obtained from the Experiments of Rectangular Tubes.

No. of experiments.	Distance between supports.	Depth of tube.	Width of tube.	Thickness of plate in inches.		Ultimate deflection.	Breaking weight.	Remarks.
				Top.	Bottom.			
	ft. in.	in.	in.	in.	in.	in.	lbs.	
14	17 6	9·6	9·6	·075	·272	1·10	3,783	Broke by compression.
14*a*	17 6	9·6	9·6	·272	·075	1·13	8,273	(Reversed). Extension.
15	17 6	9·6	9·6	·075	·142	0·94	3,788	Compression.
15*a*	17 6	9·6	9·6	·142	·075	1·88	7,148	(Reversed). Extension.
16	17 6	18·25	9·25	·059	·149	0·93	6,812	Compression.
16*a*	17 6	18·25	9·25	·149	·059	1·73	12,188	(Reversed). Compression.
17	24 0	15·00	2·25	·160	·160	2·66	17,600	Compression.
18	18 0	13·25	7·50	·142	·142	1·71	13,680	Compression.
23	18 6	13·00	8·00	·066	·066	1·19	8,812	Compression. Fin on the top.
25	11 0	8·00	1·00	·282	·116	0·75	11,254	Compression. Fin on the top.
29	19 0	15·40	7·75	·230	·180	1·59	22,467	Sides distorted. Corrugate top.

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* It is unfortunate that the exact temperature was not ascertained at the time; the atmosphere may however be taken at nearly the freezing-point early in the morning, and a few degrees under summer heat at noon.

Model Tube.

No. of experiments.	Distance between supports.	Depth of tube.	Width of tube.	Thickness of plate in inches.		Ultimate deflection.	Breaking weight.	Remarks.
				Top.	Bottom.			
	ft. in.	ft. in.	ft. in.	area.	area.		lbs.	
1	75 0	4 6	2 11	24·02	8·80	……	79,578	Broke by tension.
2	75 0	4 6	2 11	24·02	12·80	……	97,102	Twisted over.
3	75 0	4 6	2 11	24·02	12·80	……	126,128	By tension.
4	75 0	4 6	2 11	24·02	17·80	……	148,129	By tension.
5	75 0	4 6	2 11	24·02	22·45	……	129,009	Weight left suspended. Tube turned on its side.
6	75 0	4 6	2 11	……	……	……	26,781	Experiment discontinued.
7	75 0	4 6	2 11	24·02	22·45	……	135,255	Weight left suspended for nine days.
8	75 0	4 6	2 11	24·02	22·45	……	135,255	Broke by tension.
9	75 0	4 6	2 11	24·02	22·45	……	154,452	Broke by tension.
10	75 0	4 6	2 11	24·02	22·45	……	192,892	Crushed on the top.

GENERAL DEDUCTIONS FROM THE FOREGOING EXPERIMENTS.

It will be instructive to compare the weight of a tube with its breaking load.

Comparative Weights and Strengths of Cylindrical Tubes.

No. of experiments.	Distance between supports.	Weight of tube.	Breaking weight.	Ratio.	Diameter.	Thickness.
	ft. in.	lbs.	lbs.		in.	in.
1	17 0	102	3,040	1 : 29·0	12·18	·034
2	17 0	107	2,704	1 : 25·2	12·00	·037
3	15 7½	392	11,440	1 : 29·1	12·40	·131
4	23 5	334	6,400	1 : 19·1	18·26	·058
5	23 5	346	6,400	1 : 18·5	17·68	·063
6	23 5	777	14,240	1 : 18·3	18·18	·119
7	31 3¼	907	9,760	1 : 10·7	24·10	·095
8	31 3¼	1385	14,240	1 : 10·2	24·30	·095
9	31 3½	1005	10,880	1 : 10·8	24·20	·135

Comparative Weights and Strengths of Elliptical Tubes.

No. of experiments.	Distance between supports.	Weight of tube.	Breaking weight.	Ratio weight to strength.	Dimension of tube. Diameters.	
					Transverse.	Conjugate.
	ft. in.	lbs.	lbs.		in.	in.
19	17 0	109	2,100	1 : 19·2	14·62	9·25
20	24 0	708	17,076	1 : 24·1	21·66	13·50
21	24 0	357	7,714	1 : 21·7	21·25	14·12
22	18 6	232	6,867	1 : 29·6	12·00	7·50
22	18 6	232	5,648	1 : 24·3	12·00	7·50
24	17 6	374	15,490	1 : 40·0	15·00	9·75

Comparative Weights and Strengths of Rectangular Tubes.

No. of experiments.	Distance between supports.	Weight of tube.	Breaking weight.	Ratio weight to strength.	Dimensions of tube.	
					Depth.	Width.
	ft. in.	lbs.	lbs.		in.	in.
14	17 6	202	3,738	1 : 18	9·6	9·6
14*a*	17 6	384	8,273	1 : 21	9·6	
15	17 6	255	3,788	1 : 14	9·6	
15*a*	17 6	255	7,148	1 : 28	9·6	12·0
16	17 6	317	6,812	1 : 21	18·25	9·25
16*a*	17 6	317	12,188	1 : 38	18·25	9·2
17	24 0	788	17,600	1 : 22	15·0	2·25
23	18 6	267	8,812	1 : 33	13·0	8·0
29	19 0	500	22,469	1 : 50	15·4	7·75

Now it will be hereafter shown, that this ratio of the weight of a tube to its breaking weight varies directly as the depth of the tube, when its length is constant. In order therefore to ascertain the comparative strengths of these tubes, we shall reduce some of the best of each sort to the same depth.

Thus No. 4, 20, and 17 have nearly the same length; hence by reducing them to the same depth, we find their relative strengths to be as follows:—

No. 4.	No. 20.	No. 17.
10·5	11·1	14·6

Taking No. 1, 24, and 15*a*, we find

No. 1.	No. 24.	No. 15*a*.
24·4	26·6	29·1

Again, for No. 22 and 29, we find

No. 22.	No. 29.
24·6	32·4.

Hence it appears that the rectangular tubes are the best, and that the elliptical ones are the next in order.

It is desirable that we should have some formula, which will enable us to estimate the comparative strength of these tubes, whatever may be their form or relative dimensions. For this purpose it will be hereafter shown, that the following formula is approximately true for all tubular girders, viz.

$$W = \frac{AdC}{l}; \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

where W= the breaking load, A= the area of the whole cross section of the tube in square inches, d= the depth of the tube, l= the distance between the supports, and C= a constant, which must be determined by experiment for the particular *form* of the tube. Moreover the value of C, determined for different forms of tubes, will enable us to ascertain their comparative strength. From the above relation, we have

$$C = \frac{Wl}{Ad} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

Thus to find the value of this constant for Experiment 29, we have

$$W = \frac{22470 + \frac{1}{2}(500)}{2240} = 10{\cdot}142 \text{ tons},$$

$$l = 19 \times 12,\ d = 15{\cdot}4, \text{ and } A = 7{\cdot}048 \text{ square inches};$$

$$\therefore \quad C = \frac{10{\cdot}142 \times 19 \times 12}{7{\cdot}048 \times 15{\cdot}4} = 21{\cdot}3 \text{ tons}.$$

Proceeding after this manner, the following tables have been calculated.

Comparative Strengths of Tubes, indicated by the Value of C.

Cylindrical Tubes.

No. of experiments.	Breaking weight in tons, or W.	Area section, or value of A.	Value of the constant C in tons.
1	1·38	1·5612	14·8
2	1·23	1·3948	15·0
3	5·19	5·1032	15·4
4	2·93	3·3385	13·5
5	2·93	3·5048	13·3
6	6·48	6·7970	14·7
7	4·13	7·1928	9·9
8	6·66	....	9·9
9	5·08	7·4506	10·5

Elliptical Tubes.

No. of experiments.	Breaking weight in tons, or W.	Area section, or value of A.	Value of the constant C in tons.
20	7·78	7·1824	14·4
21	3·81	3·3300	11·9
22	3·12	3·3720	17·1
24	5·49	7·0010	17·8

Rectangular Tubes.

No. of experiments.	Breaking weight in tons, or W.	Area section, or value of A.	Value of the constant C in tons.
14	1·71	3·20	11·7
14*a*	3·73	5·32	15·3
15	1·74	4·04	9·5
15*a*	3·24	4·04	17·8
17	8·03	8·00	19·3
25	5·05	2·90	28·6
29	10·13	7·05	21·3
30	5·83	5·75	16·1
31	8·05	6·73	15·1
33	37·95	41·82	15·1
35	58·73	45·82	21·1
36	68·56	50·82	22·5
41	89·15	55·47	26·7

In the rectangular tubes, No. 14 *a*, 15 *a*, 17, 25, 29, 35, 36 and 41, appear to have had, nearly, a proper distribution of the material; the mean value of C estimated from these is 21·5 tons, which is considerably greater than the value of C deduced for any of the cylindrical or elliptical tubes.

	Mean value of C.
For the cylindrical tubes	=13·03 tons.
For the elliptical tubes	=15·3 tons.
For the rectangular tubes	=21·5 tons.

Hence we infer that the rectangular form of tubes is considerably stronger than either the cylindrical or elliptical form.

Proceeding with eq. (1.), after the manner of investigation given in the note at page 104, we obtain eq. (8.), page 105, which expresses the breaking load of a large tube in all respects *similar* to an experimental tube.

Strength of the Conway Tube, Experiments 41 *and* 42.

In the model tube, experiment 41, $l=75$, $L=86\cdot25$ tons, $w=\frac{5\cdot8}{2}=2\cdot9$ tons. In experiment 42, $l_1=400$, hence we have by eq. (8.), page 105,

$$L_1=\left(\frac{l_1}{l}\right)^2\left\{L-\frac{l_1-l}{l}\cdot w\right\}\text{tons}$$

$$=\left(\frac{400}{75}\right)^2\left\{86\cdot25-\frac{400-75}{75}\times 2\cdot9\right\}=2096 \text{ tons},$$

which is the breaking load of the tube, and hence the breaking weight, $W_1 = 2096 + \frac{1300}{2} = 2746$ tons.

Again, adopting formula (8.), page 276, we have $S = 26{\cdot}8$, $l = 400 \times 12$, $d = 25{\cdot}5 \times 12$, and from eq. (9.), page 276, $I = 17900000$ nearly;

$$\therefore \quad W = \frac{8SI}{ld} = \frac{8 \times 26{\cdot}8 \times 17900000}{400 \times 12 \times 25{\cdot}5 \times 12} = 2600 \text{ tons nearly,}$$

which result nearly coincides with the breaking weight before found.

Again, by formula (5.) page 275, we have $A = 1530$, $d = 25{\cdot}5$, $l = 400$, and $C = 26{\cdot}8$,

$$\therefore \quad W = \frac{AdC}{l} = \frac{1530 \times 25{\cdot}5 \times 26{\cdot}8}{400} = 2614 \text{ tons,}$$

which almost exactly coincides with the preceding result.

Deflections of the Conway Tube, Experiments 41 *and* 42.

In order to discover the law of the deflections in tubular beams, we shall first consider the table of experiment 41. Adding $\frac{5}{8}$ths of the weight of the tube to the numbers in the column of weights in the table, page 264, we have—

No. of experiments.	Deflecting weight in lbs.	Deflections in inches.
4	70,411	1·48
8	122,797	2·70
12	156,580	3·58
16	172,878	3·98
20	185,225	4·47
24	197,307	4·81

Here we find the approximate relation of the deflections and the deflecting weights to be expressed by the equality $\delta = \frac{W}{43000}$, that is, the deflection in inches is, approximately, the 43,000th part of the deflecting weight expressed in lbs. This relation is more accurately expressed by the equality, $\delta = \frac{W}{38000} - {\cdot}4$.

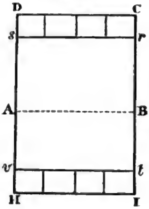
Making a similar reduction of the table in experiment 42, we have, regarding the weights as laid over the centre of the tube,

No. of experiments.	Deflecting weight in tons.	Deflections in inches.
1	812	7·91
2	907	9·02
3	966	9·50
4	1013	10·50
5	1113	10·95

Here the deflections in inches are pretty nearly the 101st part of the deflecting weights expressed in tons. Taking the breaking weight, as determined at page 272, to be 2600 tons, we have the ultimate deflection of the tube $=\frac{1}{101}$ of 2600 $=25·7$ inches. Now in order to test the accuracy of these calculations, we have for similar tubes (see formula (3.), page 165) the following relation, $\frac{\delta}{\delta_1}=\frac{l}{l_1}$; that is, in the present case, $\frac{\delta}{4·81}=\frac{400}{75}$; therefore $\delta=25·6$ inches, where the same result is obtained by two processes perfectly independent of each other.

Investigation of formulæ relative to Rectangular Tubes.

Fig. 71.



Let HICD represent a section of the tube with a cellular structure; AB the neutral axis;

W = the breaking load of the tube;
l = the distance between the supports;
e = Ds, the depth of the top cells;
e_1 = Hv, the depth of the bottom cells;
p = the whole breadth of the spaces in the top cells;
k = the thickness of the plates;
a = the area of the section of the material in the top cells DCrs.
a_1 = the area of the section of the material in the bottom cells HItv; and so on to similar notation for the parts below the neutral axis;

f = the force per square inch, opposed to compression at the centre of the top cells;

S = the force per square inch, opposed to compression at the upper edge DC;

g = the distance of the centre of the top cells from the neutral axis AB;

h = AD, the distance of the top of the tube from the neutral axis;

G = the distance between the centres of the top and bottom cells;

M = the moment of resistance of the section HICD to rupture.

Hence we have, neglecting the material in the sides *vs* and *tr*, resistance of material in DC*rs* to compression = resistance whole area DC*rs* — resistance space in the cells

$$=\frac{bf}{g}\int_{g-\frac{e}{2}}^{g+\frac{e}{2}}\{xdx\}-\frac{pf}{g}\int_{g-\frac{e}{2}+k}^{g+\frac{e}{2}-k}\{xdx\}$$

$$=\frac{bf}{g}\Big\{\Big(g+\frac{e}{2}\Big)^2-\Big(g-\frac{e}{2}\Big)^2\Big]-\frac{pf}{2g}\Big\{\Big(g+\frac{e}{2}-k\Big)^2-\Big(g-\frac{e}{2}+k\Big)^2\Big\}$$

$$=\frac{bf}{2g}\cdot 2ge-\frac{pf}{2g}\cdot 2g(e-2k)$$

$$=f\{be-p(e-2k)\}=fa.$$

Similarly, we have

Resistance of material in HI*tv* to extension $=f_1a_1$;

$$\therefore\quad fa=f_1a_1\quad .\quad .\quad .\quad (1.)$$

Moment resistance of DC*rs* to compression, m

$$=\frac{fb}{g}\int_{g-\frac{e}{2}}^{g+\frac{e}{2}}\{x^2dx\}-\frac{fp}{g}\int_{g-\frac{e}{2}+k}^{g+\frac{e}{2}-k}\{x^2dx\}$$

$$=\frac{fb}{3g}\Big\{\Big(g+\frac{e}{2}\Big)^3-\Big(g-\frac{e}{2}\Big)^3\Big]-\frac{fp}{3g}\Big\{\Big(g+\frac{e}{2}-k\Big)^3-\Big(g-\frac{e}{2}+k\Big)^3\Big\}$$

$$=fbeg\Big\{1+\frac{e^2}{12g^2}\Big\}-fp(e-2k)g\Big\{1+\frac{(e-2k)^2}{12g^2}\Big\}.$$

Without infringing upon the peculiarity of the structure, we may suppose that k is indefinitely small, and that the material is chiefly collected in the vertical plates connecting DC and *sr*, then we have

$$m=\{fbeg-fp(e-2k)g\}\left(1+\frac{e^2}{12g^2}\right)$$

$$=fg\{be-p(e-2k)\},\ \text{neglecting } \frac{e^2}{12g^2}{}^{*}$$

$$=fag.$$

Similarly, we have

Moment resistance of HI*tv* to extension $=f_1a_1g_1$,

$$\therefore\quad \mathrm{M}=fag+f_1a_1g_1\,;$$

but by eq. (1.) $fa=f_1a_1$,

$$\therefore\quad \mathrm{M}=fa(g+g_1)$$

$$=fa\mathrm{G} \text{ or } f_1a_1\mathrm{G}\,; \quad . \ . \ . \ . \quad (2.)$$

$$\therefore\quad \frac{\mathrm{W}l}{4}=fa\mathrm{G},$$

$$\therefore\quad \mathrm{W}=\frac{4fa\mathrm{G}}{l}, \text{ or } \frac{4f_1a_1\mathrm{G}}{l} \quad . \ . \ . \quad (3.)$$

Now, in similar tubes f must be constant, for $f=\mathrm{S}\times\frac{g}{h}=\mathrm{S}\times$ a constant; and moreover G must be some constant proportional part of the depth of the tube; hence $4f\mathrm{G}=d\times$ a constant $=d\mathrm{C}$,

$$\therefore\quad \mathrm{W}=\frac{ad\mathrm{C}}{l}, \quad . \ . \ . \ . \ . \ . \ . \ . \ . \quad (4.)$$

which is the relation (1) assumed at page 68, where C must be determined by experiment, as explained at page 68.

Moreover, as the area of the whole section of the material in similar tubes must be some constant proportional part of a, we also have

$$\mathrm{W}=\frac{\mathrm{A}d\mathrm{C}}{l}, \quad . \ . \ . \ . \ . \ . \ . \ . \ . \quad (5.)$$

---

[*] In the model tube, experiment 41,

$$e=6{\cdot}5,\quad g>27-3{\cdot}2>23{\cdot}8,$$

$$\therefore\quad \frac{e^2}{12g^2}<\frac{6{\cdot}5^2}{12\times 23{\cdot}8^2}<\frac{1}{160}.$$

Hence it appears that the portion of the above formula which is rejected is less than $\frac{1}{160}$th part of that which is retained, so that the relation $m=fag$ is sufficiently exact for all practical purposes.

whence we have for the value of the constant,

$$C=\frac{Wl}{Ad} \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

The general formula (5) will afterwards be shown to hold true for cylindrical and elliptical tubes.

In the model tube, Experiment 41, the neutral axis must, obviously, lie very nearly in the centre of the section. It will therefore be interesting to investigate a formula for this case. For this purpose, let I = the moment of inertia of the section HICD, and k_2 = the sum of the thicknesses of the side plates *vs* and *tr*, then we have, by a well-known formula,

$$\frac{Wl}{4}=\frac{SI}{h} \quad . \quad . \quad . \quad . \quad (7.)$$

$$\therefore \quad W=\frac{8SI}{ld} \quad . \quad . \quad . \quad . \quad (8.)$$

But the moment of inertia of the section $=2\{$moment ABCD — moment space AB*rs* — moment space in the cells DC*rs*$\}$;

$$\therefore \ I=2\left[b\int_0^h\{x^2dx\}-(b-k_2)\int_0^{h-e}\{x^2dx\}-p\int_{h-e+k}^{h-k}\{x^2dx\}\right]$$

$$=\frac{2}{3}\left[bh^3-(b-k_2)(h-e)^3-p\{(h-k)^3-(h-e+k)^3\}\right] \ . \ . \ (9.)$$

which expresses the value of I in eq. (8.).

The value of the constant S deduced from (7.) is

$$S=\frac{Wlh}{4I}$$

$$=\frac{Wld}{8I} \quad . \quad . \quad . \quad . \quad (10.)$$

As an application of eq. (6.) and (10.), let us take the data of Experiment 41.

Ex. In Experiment 41, $W=89{\cdot}15$ tons, $A=55{\cdot}47$, $d=4{\cdot}5$ft., $l=75$ ft., $b=35$ in., $k={\cdot}147$ in., $k_2={\cdot}198$ in., $e=6\frac{1}{2}$ in.

Whence we have from eq. (6.),

$$C=\frac{Wl}{Ad}=\frac{89{\cdot}15\times75}{55{\cdot}47\times4{\cdot}5}=26{\cdot}7 \text{ tons.}$$

And from eq. (10.), we have

$$b-k_2=35-\cdot198=34\cdot802,\ h=\frac{4\cdot5\times12}{2}=27,$$

$$h-e=27-6\cdot5=20\cdot5,\qquad p=35-7\times\cdot147=33\cdot971,$$

$$h-k=27-\cdot147=26\cdot853,\ h-e+k=20\cdot5+\cdot147=20\cdot647,$$

$$\therefore\ \mathrm{I}=\frac{2}{3}[35\times27^3-34\cdot802\times20\cdot5^3-33\cdot971\{26\cdot853^3-20\cdot647^3\}]$$

$$=20200 \text{ nearly;}$$

$$\therefore\quad \mathrm{S}=\frac{\mathrm{W}ld}{8\mathrm{I}}=\frac{89\cdot15\times75\times12\times4\cdot5\times12}{8\times20200}=26\cdot8 \text{ tons.}$$

Here it will be seen that there is a very near coincidence between the values of C and S.

When the tube is simply a hollow rectangular beam, similar to that of experiment 14, we find from equations (8.) and (9.), by making $e=2k$,

$$\mathrm{I}=\frac{2}{3}\{bh^3-(b-k_2)(h-2k)^3\};$$

substituting $\frac{d}{2}$ for h, putting b_1 for $b-k_2=$ the internal breadth, and d_1 for $d-4k=$ the internal depth, we have

$$\mathrm{I}=\frac{1}{12}\{bd^3-b_1d_1^3\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11.)$$

$$\therefore\quad \mathrm{W}=\frac{8\mathrm{SI}}{ld}$$

$$=\frac{2\mathrm{S}}{3ld}\{bd^3-b_1d_1^3\}$$

$$=\frac{2\mathrm{S}}{3ld}\{bd^3-(b-2k)(d-2k)^3\}$$

$$\therefore\quad \mathrm{S}=\frac{3\mathrm{W}ld}{2\{bd^3-(b-2k)(d-2k)^3\}} \quad . \quad . \quad . \quad . \quad (12.)$$

by substituting $b-2k$ for b_1, and $d-2k$ for d_1.

Ex. In the second experiment recorded in the table at p. 46, $l=30\times12$, $\mathrm{W}=\frac{1\cdot212}{2}+22\cdot75=23\cdot356$ tons, $d=24$, $b=16$, and $k=\cdot272$, hence we have by equation (12.),

$$\mathrm{S}=\frac{3\times23\cdot356\times30\times12\times24}{2\{16\times24^3-(16-\cdot544)(24-\cdot544)^3\}}=14 \text{ tons nearly.}$$

Formulæ relative to Cylindrical Tubes.

As the thickness of the metal in these tubes is uniform, we shall suppose that the neutral axis passes through the centre of the circular section.

Let $r, r_1 =$ the radii of the exterior and interior circles respectively;
$d, d_1 =$ the diameters of the exterior and interior circles respectively;
$k =$ the thickness of the metal;
$A =$ the area of the section of the material;
$x, y =$ the coordinates of a point in the circle, referred to the centre as the origin.

The other notation being the same as in the preceding investigations; then we have

$$
\begin{aligned}
M &= \frac{2S}{r}\int_{-r}^{r}\{yx^2dx\} - \frac{2S}{r}\int_{-r_1}^{r_1}\{yx^2dx\} \\
&= \frac{2S}{r}\cdot\frac{\pi r^4}{8} - \frac{2S}{r}\cdot\frac{\pi r_1^{\,4}}{8} \\
&= \frac{S\pi}{4r}\{r^4 - r_1^{\,4}\} \\
&= \frac{S\pi}{4r}(r^2 - r_1^{\,2})(r^2 + r_1^{\,2}) \\
&= \frac{rS}{4}(\pi r^2 - \pi r_1^{\,2})\left\{1 + \left(\frac{r_1}{r}\right)^2\right\}, \\
&= \frac{rSA}{4}\left\{1 + \left(\frac{r_1}{r}\right)^2\right\}.
\end{aligned}
$$

Now in similar tubes $\frac{r_1}{r}$ is a constant quantity, and consequently $1 + \left(\frac{r_1}{r}\right)^2$ is also a constant quantity; in this case therefore we have

$$M = \frac{AdC}{4},$$

$$\therefore \quad \frac{Wl}{4} = \frac{AdC}{4},$$

$$\therefore \quad W = \frac{AdC}{l} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13.),$$

which is the same general formula as that given in equation (5.)

When the thickness of the tube is small as compared with its depth, then $1+\left(\frac{r_1}{r}\right)^2=2$ very nearly*, and in this case,

$$\therefore \quad \mathrm{M}=\frac{r\mathrm{SA}}{4}\left\{1+\left(\frac{r_1}{r}\right)^2\right\}=\frac{r\mathrm{SA}}{2},$$

$$\therefore \quad \mathrm{W}=\frac{\mathrm{A}d\mathrm{S}}{l};$$

comparing this expression with (13.), we find $\mathrm{C}=\mathrm{S}$.

Formulæ relative to Elliptical Tubes.

Let a = semi-axis major of the exterior ellipse;
b = semi-axis minor of the exterior ellipse;
a_1 = semi-axis major of the interior ellipse;
b_1 = semi-axis minor of the interior ellipse;
d = the depth of the tube;
k = the thickness of the metal;
A = the area of the section of the material; &c.

Proceeding precisely as in the case of the cylindrical tubes, we have

$$\mathrm{M}=\frac{\mathrm{S}\pi}{4a}\{ba^3-b_1a_1^3\};$$

now we shall assume that the exterior and interior ellipses in the transverse section of the tube are similar; hence in this case $\frac{a}{b}=\frac{a_1}{b_1}$;

$$\therefore \quad \mathrm{M}=\frac{\mathrm{S}\pi}{4b}\{b^2a^2-b_1^2a_1^2\}$$

$$=\frac{\mathrm{S}\pi}{4b}(ba-b_1a_1)(ba+b_1a_1)$$

---

* In experiment 4, $d=18{\cdot}26$, $k={\cdot}0582$;

$$\therefore \quad r=\frac{18{\cdot}26}{2}=9{\cdot}13,\ r_1=9{\cdot}13-{\cdot}0582=9{\cdot}072,$$

and $\quad 1+\left(\frac{r_1}{r}\right)^2=1+\left(\frac{9{\cdot}072}{9{\cdot}13}\right)^2=1{\cdot}99$ or 2 nearly.

$$=\frac{aS\pi}{4}(ba-b_1a_1)\left(1+\frac{b_1a_1}{ba}\right)$$

$$=\frac{aSA}{4}\left\{1+\frac{b_1a_1}{ba}\right\}.$$

Now in similar tubes $\frac{b_1a_1}{ba}$ is a constant quantity,

$$\therefore \quad M=\frac{AdC}{4},$$

$$\therefore \quad \frac{Wl}{4}=\frac{AdC}{4},$$

$$\therefore \quad W=\frac{AdC}{l}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

which is the same general formula as that given in equations (5.) and (13.)

When the thickness of the tube is small as compared with its depth, then $1+\frac{b_1a_1}{ba}=2$ very nearly*, and in this case,

$$\therefore \quad M=\frac{aSA}{4}\left\{1+\frac{b_1a_1}{ba}\right\}=\frac{aSA}{2},$$

$$\therefore \quad W=\frac{AdS}{l};$$

comparing this expression with (14.), we find C=S.

From equations (5.), (13.) and (14.), it appears that

$$W=\frac{AdC}{l}$$

is a general expression for the breaking weight of all tubes, whether rectangular, cylindrical, or elliptical, under the limits specified; where A is the area of the section of the material in square inches, d the depth in linear inches, l the distance between the points of support in linear inches, and C a constant determined by experiment for the particular form of the tube.

---

* In experiment 19, $a=\frac{14{\cdot}62}{2}=7{\cdot}31$, $b=\frac{9{\cdot}25}{2}=4{\cdot}625$, $k={\cdot}0416$,

$a_1=7{\cdot}31-{\cdot}0416=7{\cdot}2684$, $b_1=4{\cdot}625-{\cdot}0416=4{\cdot}5834$,

and $1+\frac{b_1a_1}{ba}=1+\frac{4{\cdot}5834\times 7{\cdot}2684}{4{\cdot}625\times 7{\cdot}31}=1{\cdot}98$ or 2 nearly.

Hence the value of the constant C, in those expressions, may be taken as the index of the comparative strengths of the different kinds of tubes.

To express the Breaking Weight of a Tube as compared with the Weight of the Tube itself.

Let $s =$ the weight of cubic foot of wrought-iron, and $w =$ the weight of the tube; then supposing the tube to be uniform in its dimensions, we have

$$W = \frac{AdC}{l} = 144Als \cdot \frac{d}{l^2} \cdot \frac{C}{144s}$$

$$= w \cdot \frac{d}{l^2} \cdot \frac{C}{144s},$$

$$\therefore \quad \frac{W}{w} = \frac{d}{l^2} \cdot \frac{C}{144s} = \frac{dC_1}{l^2},$$

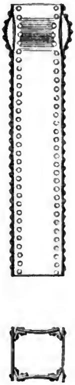
where the ratio varies as the depth of the tube, and inversely as the square of the length.

EXPERIMENT ON THE RESISTANCE OF A RECTANGULAR TUBE TO A CRUSHING FORCE.

Not having in my possession the experiments of Mr. Hodgkinson on the resistances of tubes of different forms to a crushing force, I considered it absolutely necessary for my own guidance to institute an experiment, upon a large scale, in order to ascertain the resistance offered by a rectangular malleable iron tube or cell (of nearly the same dimensions as those in the Britannia and Conway tubes), to a force tending to crush it.

The experiment was made upon a tube 18 inches square, 8 feet long, and composed of plates half an inch thick, riveted to angle-iron, as shown in the annexed sketch.

Fig. 72.



The tube or cell was placed vertically under a powerful hydraulic press belonging to Messrs. Benjamin Hick and Co., Bolton, Mr. John Hick, the active partner, having kindly offered the use of the pumps and apparatus for that purpose.

The hydraulic press was one of the best construction; it had four distinct cylinders and rams, and the pressure was indicated by an exceedingly sensitive lever with a carefully graduated scale, on which the strain during the different stages of the experiment was recorded. The following table exhibits the pressure and flexure of the tube under the influence of the force applied.

Experiment XLIII.—May 7th, 1847.

Rectangular tube or cell, 8 feet long, 1 foot 6 inches square, and composed of plates and angle-iron, on all the four sides, half an inch thick.

Sectional area of the cell =50 inches.

No. of experiments.	Pressure in tons.	Flexure on the side A.	Flexure on the side B.	Compression from force applied in inches.	Remarks.
1	115	......	......	......	The discrepancies exhibited on the sides were in a great measure occasioned by the "buckling" of the plates, which produced contortions on the surface.
2	165	·25	·20	·000	
3	215	·25	·48	·000	
4	265	·25	·70	·000	
5	315	·25	·70	·025	
6	365	·25	1·20	·032	
7	415	·25	·95	·032	
8	465	·25	·70	·030	
9	515	·50	·95	·042	
10	565	·75	·95	·061	
11	615	·75	1·75	·063	
12	665	1·00	......	·160	
13	690	......	......	......	A short time before the whole amount of pressure came upon the tube it was progressively yielding to the strain. It ultimately became distorted by the puckering of three of the sides, as exhibited in the drawing.

Taking 680 tons as the crushing force, we have the resistance of compression per square inch $=\frac{680}{50}$ tons $=13{\cdot}6$ tons. This result nearly coincides with the value of S determined for the thinner tubes with single plates at the top and bottom (see example, p. 277). But in all the tubes with a cellular structure at the top, the value of S is considerably greater than the resistance to compression indicated by this experiment (see example, p. 276). The combination of cells, taken in connection with the peculiar mode in which the crushing and tensile forces are applied, probably contributes to the great strength observed in the tubes with a cellular structure.

The total force of resistance presented by the cellular top of the Conway tube $=670\times13{\cdot}6=9112$ tons.

Upon the whole, however, this experiment tended to give us additional confidence in the security of the structure.

EXPERIMENTS TO DETERMINE THE STRENGTH OF RIVETS WHEN INSERTED IN LINES ON EACH SIDE OF THE JOINT, AS PER SKETCH.

Fig. 73.

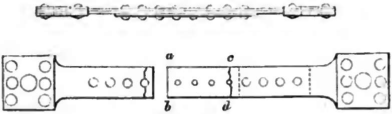
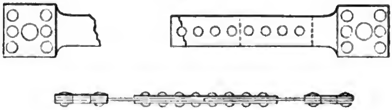


Fig. 74.



Entertaining perfect confidence in the resisting powers of the upper side of the tube to compression, the next consideration was to establish, by direct experiment, the same confidence in the resistance of the bottom to tension. The principal cause of doubt in reference to this part of the construction was occasioned by the perforations for the rivets weakening the plates. How to remedy this defect was a question of some difficulty, and it required more than ordinary consideration in adopting the means necessary for attaining the construction of equal strengths. The mode of riveting materially affected the strengths, as well as the proportions of the bridge.

Double, treble, and quadruple riveting was thought of; but one after another were abandoned on account of the rivet-holes weakening the plates, and I should have almost despaired of attaining the object in view, but for the principle of longitudinal or chain-riveting having occurred to me, after repeated trials of other modes and forms. Having satisfied myself as to the feasibility of this system of riveting, it was still desirable that it should be submitted to the test of experiment. This was done upon two distinct methods of jointing, one with a single thickness of plates, similar to the vertical divisions of the cells in the two large tubes, and the other of a double thickness of plates, similar to the upper and lower platforms of the bottoms of the tubes. The jointed plates having been prepared, the experiments were conducted by a powerful lever, tearing the joints and plates asunder in the direction of the line of the rivets as follow :—

Chain-Riveting Single Plates with double covers over the Joint.

Area of section through the solid plate at *a*, *b* . . . . } 3·5 × ·25 = ·875 inch.
Area of the covering plates . . . 3·5 × ·26 = ·910 „
Area of section through the rivet-holes at *c*, *d* . . . . } 3 × ·25 = ·750 „

Diameter of the rivets, each ½-inch, four on each side of the joint, as exhibited above.

No. of experiments.	Weight in lbs.	Elongation in inches.	Remarks.
1	5,600	....	Weight of the lever.
2	26,656		
3	28,448		
4	30,240	·021	
5	32,032	·034	
6	33,824	·034	
7	35,626	·044	
8	37,418	·052	
9	39,210	·056	
10	41,002	....	Torn asunder through the hole at *c*, *d*, after sustaining the load a few seconds.

If we take the area of the plate at the point of fracture = ·750 inch, it will be found that it required a power of 24·41 or nearly 24½ tons per square inch to tear it asunder.

The next experiment was with double plates and a single cover over the joint similar to the tensile joints of the two bottom platforms of the Britannia and Conway tubes, as follows :—

Area of section through the line *a*, *b* = ·875 × 2 = 1·750 inch.
Area of section through the rivet-holes at *c*, *d* = 1·5 „
Area of covering plate through the rivet-hole = ·91 „
Rivets as before, ½-inch diameter.

No. of experiments.	Weight in lbs.	Elongation in inches.	Remarks.
1	5,600	......	Weight of lever.
2	26,656	·016	
3	37,408	·025	
4	46,368	·028	
5	55,328	·057	
6	62,496	·100	
7	69,664	......	Broke by shearing off the rivets close to the plate, as shown in the drawing.

From the above experiment it appears that fracture took place through the solid plate on one side, and by shearing off the rivets on the other. Hence the area of the section of fracture $=\cdot 875+\cdot 785=1\cdot 66$ inches, and proceeding as before, we have for the breaking weight a force of 18·73 tons per square inch.

Finding the resisting powers of the rivets unequal to the strength of the double plates, they were afterwards increased from half an inch to five-eighths of an inch in diameter; or until the area of the rivets approached nearly to the area of the plates, which gave the required strength. In joints of this description exposed to a direct tensile strain, it will be found that the resisting power of rivets is nearly equal to that of the plates: or in other words, the resisting power of the rivets is to that of the plates as their respective sectional areas, that of the plates being taken through the rivet-holes.

In the single-jointed plates the half-inch rivets were more than sufficient to cause the plates to be torn asunder; whereas in the double plates the same strength of rivets was unequal to the strain; the respective areas of the first were in the ratio of ·785 : 750, and the latter of ·785 : 1·500*. These experi-

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* The strength of plates and their riveted joints is not a new subject; as ten years ago I was closely engaged in researches into the strength of iron as a material for ship-building, including the comparative value of the riveted joints of plates. These experiments are not yet published, but the following extract from the Report of the Tenth Meeting of the British Association for the Advancement of Science, held at Glasgow in 1840, bears so directly upon the question now under consideration, that we may safely refer to the abstract as given to the meeting on that occasion :—

"The number of vessels which of late years have been made entirely of mental facts were of great value in relation to the construction of the Conway tube, inasmuch as they caused us to increase the section of the rivets to nearly the same sectional area as the

iron, and the probability of the greatly extended use of this material in ship-building, renders the extension of our knowledge an object of some importance, at least, so far as to make us better acquainted with its powers of resistance to the various strains to which it is subjected in this new application. To meet these requirements the following experiments have been undertaken, and are now in a great measure completed. 1st. A series of experiments on the strength of malleable iron plates as regards a direct tensile strain, both in direction of the fibre and across it. 2nd. On the strength of the joints in plates riveted together, and on the best modes of riveting. 3rd. On the strength of the various forms of ribs or frames used in ship-building, whether wholly composed of iron, or of iron and wood.

"*On the Strength of Iron Plates.*

" In the experiments all the plates were of uniform thickness; their ends had plates riveted to them on both sides, with holes bored through them perpendicular to the plate, in order that they might be connected to shackles by bolts to tear them asunder in the middle, which were made narrower at those parts for that purpose. The results were as follows:—

" Mean breaking weights in tons per square inch when drawn in the direction of the fibre:

Yorkshire plates.........	25·77	Mean, 22·52 tons.
Yorkshire plates.........	22·76	
Derbyshire plates	21·68	
Shropshire plates	22·83	
Staffordshire plates......	19·56	

"Mean breaking weights in tons per square inch when drawn across the fibre:

Yorkshire plates.........	27·49	Mean, 23·04 tons.
Yorkshire plates.........	26·04	
Derbyshire plates	18·65	
Shropshire plates	20·00	
Staffordshire plates......	21·01	

"The foregoing experiments show that there is little or no difference in the strength of iron plates, whether drawn in the direction of the fibre or across it.

" Mr. Fairbairn then gave the results of a long series of experiments on the strength of riveted plates. The same description of plates was here used as in the previous experiments; they were, however, made wider than the former ones, in order that they might contain—after the rivet-holes were punched out—the same area of cross section as the previous ones.

plates in all the double joints, especially in the bottom of the large tubes; and the same principle must be observed in the construction of all tubular girders subjected to severe tensile strain.

"Mean breaking weights in lbs. from four plates of equal section, riveted by a single row of rivets:

20,127
16,107
18,982 } Mean, 18,590 lbs.
19,147

"The mean breaking weights in lbs. from four plates of equal sections to the last, but united with a double row of rivets:

22,699
23,371
20,059 } Mean, 22,258 lbs.
22,902

"Whence the strength of single to double riveting is as 18,590 : 22,258. But from a comparison of the results taken from the whole experiments, the strength derived from the double-riveted joints was to that of the single-riveted as 25,030 : 18,591, or as 1000 : 742.

"Comparing the strength of plates alone with that of double- and single-riveted joints, their relative values were nearly as under:—

For the strength of the plate..................	100
For that of double-riveted joints............	70
And for the single-riveted joints............	56

"Hence the strength of the plates to that of the joints are as the respective numbers 100, 70, and 56. A table was then given containing the dimensions and distances of rivets for joining together different thicknesses of plates."

The above extract will show, that considerable progress had been made in a similar inquiry to that contained in the preceding experiments, as far back as the year 1838, which was the time when the experiments on the strength of wrought iron plates and their riveted joints were commenced. Since then the subject has been laid aside in consequence of the pressure of other important duties, which for the last ten years have occupied nearly the whole of my time. I hope, however, that I shall shortly be able to complete the experiments, and, by a careful record of facts, to obtain such results as may be useful in effecting increased economy and increased security in the *construction of iron ships, and other structures.*

LETTERS FROM MR. H. ROSS, MR. J. GRAHAM, AND MR. R. MURRAY.

39 Bloomsbury Square,
London, September 28th, 1846.

Dear Sir,

In answer to your letter received this morning requesting me to state what took place at Millwall between yourself and Mr. Hodgkinson respecting the corrugated form of tube, and who suggested that form, I regret to say that I have no recollection of any particular conversation on the subject, but I feel satisfied that the subject of the corrugated iron was started by yourself, and that after the first experiments on the rectangular tubes were in hand, and the results of some of them known, you spoke to Mr. Hodgkinson of your intention to adopt or make a trial with corrugated iron, as most likely to give additional strength; and subsequently you drew the chalk sketch for Graham to go by in making the spectacle top tube. Nothing that then took place struck me as if it emanated from Mr. Hodgkinson; and as far as I recollect, he merely coincided with you in the probability of its answering better. As to what may have taken place elsewhere between yourself and Mr. Hodgkinson, of course I know nothing; but I have always been under the impression that the application of corrugated iron was your sole suggestion.

In corroboration of this impression, I have also a distinct recollection of calling on my brother, to say that all the tubes then made were experimented upon, and that you suggested one with a corrugated top; the consequence would be that weeks would elapse before the remainder of the experiments could be made; he said the bridge ought to be in hand by that time.

The experiments on the corrugated tube were not made until the 14th of October, 1845.

Trusting this explanation will satisfy all parties,

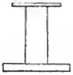
I remain, dear Sir, faithfully yours,

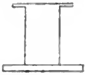
William Fairbairn, Esq., Manchester. Hn. Ross.

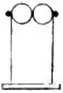
Millwall, London, September 30, 1846.

Mr. Fairbairn, Sir,

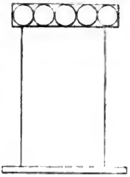
With regard to my recollection of the different sketches which I remember of your making towards the latter end of the first experiments that were made last year at Millwall, when you came up to break the beams and tubes, thus,







When we had finished you called me up to the end of the mould-loft to give me some further instruction, and when I came, you were busy sketching different forms of tubes to the gentlemen present; and among the rest was one such as this, which was considered the best by the gentlemen present at the time.



The above is the principal of my recollection.

I am, yours most truly,

James Graham.

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3 Park Place, Greenwich, May 27th, 1848.

My dear Sir,

Not having observed in the newspapers the speech to which you refer in your note of the 24th, I am much surprised by what you state as to Mr. Robert Stephenson's claiming the whole merit of the Conway Bridge.

I quite agree with you in thinking that your father must have borne a very great share of the responsibility and odium, had the tubular system proved a failure, and it seems very illiberal in Mr. Stephenson now to lay claim to a successful result, which has been obtained simply by acting up to the facts and laws affecting the strength of wrought-iron tubes, first established by the Millwall experiments. I always understood that the whole conduct of these experiments was left to Mr. Fairbairn, and they appeared to me to be alike planned and executed by him. And I do not even remember of Mr. Stephenson being present at these experiments, or making any suggestions with reference to them until the best form of tube had been established. I have now before me the whole series of the experiments (in which you are aware I took an active part), with the deflections and breaking weights of tubes of twenty-four different varieties; also sundry sketches and directions in your father's own hand-writing as to the construction of some of the tubes, and it appears to me quite plain that the present form of the Menai and Conway Bridge has been progressively arrived at by means of those experiments solely. You will observe they were made first upon circular tubes, then elliptical, square, rectangular, and finally upon rectangular with the top strengthened, either by an additional plate or by double corrugated plates, which seems the nearest approach that could be made in experiments of that scale, to the cellular form employed first in the experimental bridge at Millwall, and subsequently for the Menai and Conway.

Believe me, my dear Sir,
Yours very truly,

Thomas Fairbairn, Esq. ROBERT MURRAY.

Printed by R. and J. E. Taylor, Red Lion Court, Fleet Street.

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